JOURNAL

OF

THE FRANKLIN INSTITUTE

OF THE

State of Pennsylvania,

AND

AMERICAN REPERTORY.

JULY, 1843.

Civil Engineering.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

On Cast and Rolled Iron Rails for Railways. *By* JOHN C. TRAUTWINE, *Civil Engineer and Architect.*

Although this subject has recently been much discussed, it does not appear to me that all the facts bearing upon it have been taken into consideration; or that all those which have been examined, have received that degree of attention which is requisite to a satisfactory solution of the question, which of these two materials is the more eligible for common railroad purposes?

It is not my intention here to enter upon an examination of all the various points of inquiry necessarily involved in a detailed investigation of this important question; but merely to draw attention to one or two matters connected with it, which have, as I conceive, either been overlooked by those who have hitherto had the subject under consideration, or have been deemed by them of less moment than I suppose them to be.

It is my opinion that injury has resulted from the fact that those gentlemen who have been most active in discussing the question, have, in too unqualified a manner, expressed themselves in favor of one material, or the other, as preferable *in all cases*, to the entire exclusion of the other: or, in other words, I conceive the question to be not an *abstract* one, but one dependent upon contingent circumstances, by which it becomes so modified as to render the one material preferable, in some cases; and the other, in others. I have myself recommended the adoption of a heavy cast-iron U-rail, on a

railroad nearly one hundred miles in length, under my charge, from considerations of economy of original outlay, which attended that particular instance, and which happened to be, in that case, one of vital importance; combined with the peculiar difficulty that would necessarily be encountered in fabricating a rolled rail, in a section of country where it would be nearly impossible to obtain, or rather to retain, a sufficient number of experienced workmen for conducting an extensive rolling establishment uninterruptedly. Those who have embarked in such undertakings, under similar circumstances, can testify from experience how much importance is to be attached to this consideration.

Added to these reasons, it was my intention to employ engines with much less weight upon their drivers than is usual on most of our railroads; a point which, as I hope to show further on, has much to do in deciding the question of a choice between cast and rolled rails.

Those who advocate the future rejection of rolled rails "in toto," and the consequent adoption of cast-iron, urge, as the chief argument in support of their views, the splitting, or *lamination*, as it is more generally termed, which experience has shown to take place, to a very serious extent, in the rolled rails used on all our railroads.

Instead of lasting from forty, to one hundred years, which was the term of duration originally predicted for the rolled rail, the supposition being based upon the probable rate of wear, or diminution of material, as deduced from some limited experiments, it has, in fact, on most of our roads doing a heavy business, needed partial replacements within some six or eight years, with a prospect of requiring much more extensive renewals within a few years more.

I conceive, however, that this defect in the rolled rail is not one inherent in the nature of the material, but one admitting of remedies of easy application; and I will proceed to point out in what particulars I think changes should be made in the manufacture of the rolled rail, in order to diminish, if not entirely to remove, its liability to split.

In the manufacture of this rail, as is well known, the process of *piling* is necessarily employed; that is, a number of short, flat, pieces of iron are *piled* on top of each other, and the *pile* is placed in a heating furnace, where it is subjected to a temperature sufficient to bring the several pieces of which it is composed, to the welding point. When the pile has attained this point, it is withdrawn from the furnace, and, without loss of time, is rapidly passed through the successive rolls, by which its pieces are at the same time welded together, elongated, and shaped to the required section of the rail.

But, from want of proper attention on the part of the workmen, the pile is occasionally taken from the furnace before it has reached the welding point; and the consequence is, that the process of rolling does not always unite, or weld together, the several pieces into one mass, so effectually as is necessary for a good rail; and we therefore see that the weight of the engines afterwards separates them, or splits the rail.

I say the weight of the *engines*, because I am perfectly convinced, from close observation, that it is the great weight borne by their driving wheels, that does most of the injury. I have seen comparatively very little of this lamination in either edge-rails, or the common flat-bar, $\frac{5}{8}$ of an inch thick, where horses are used for the motive power, drawing cars having no greater loads than $1\frac{1}{2}$ tons to a wheel; on the contrary, I have seen the five-eighths flat-bar laid even immediately upon *continuous granite sills*, (on which the splitting is more apt to occur than in any other situation,) which have for nearly ten years endured the passage of a very heavy traffic, not drawn by locomotives, without the splitting of a single bar; while the edge-rails on the adjoining parts of the road, over which precisely the same transportation has passed, drawn by locomotives, are split to a very serious extent.

During a personal examination of most of the railroads in the United States, this matter has, among others, received my close attention; and I feel confident that this lamination may be almost, if not entirely, prevented; 1st, (and principally,) by more care in the application of the welding heat;—2d, by so passing the pile through the rolls, that the finished rail shall always consist of laminæ lying parallel to the top and bottom of the rail, as in fig. 1, instead of perpendicular to them, as in fig. 2; and, 3d, by rounding off more boldly the edges of the upper table, or top of the rail. Of the first precaution, we have already spoken. As to the second, it appears to me that the arrangement of the laminæ indicated in the first figure, must, for reasons too evident to require explanation, present more resistance to splitting (though probably less to exfoliation) than that exhibited in the second figure; and this supposition is sustained by the fact, that the splitting does almost invariably occur in lines approaching to the vertical. From this fact, the deduction very naturally follows, that a different disposition of the laminæ would produce a different result; and it is perhaps owing, in a great measure, to the frequent occurrence (probably accidental) of a horizontal disposition of them, that we have not much more splitting than actually does occur.

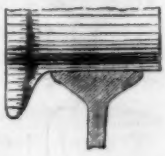
Fig. 1.

Fig. 2.

I have, it is true, in some rare instances, seen splitting occur in lines forming nearly a semi-circle, transversely of the rail; but these, I am inclined to believe, may be fairly ascribed to a very bad condition of the welding.

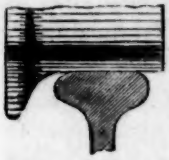
The necessity of the third precaution, or that of giving a bolder curvature to the upper edges of the rails, has also been impressed upon me by extensive observation.

Fig. 3.



It is plain that a great weight acting on a rail, the edges of whose top table are comparatively sharp, as in this figure, (3,) has a tendency to split them off; and when it happens, as it frequently does, that the line of welding of two of the original pieces of the pile lies near either of these edges, experience has shown that, if the welding be at all defective, such a splitting actually does take place.

Fig. 4.



But, if we round off these edges, as in fig. 4, it is equally apparent that this splitting tendency must be materially diminished, inasmuch as the cohesion of a much more extended area must be overcome, before a piece can be separated.

I have universally observed so much less splitting in such rails as are boldly rounded at the top edges, than in those whose edges were comparatively sharp, that I have no doubt but that a still further extension of the same principle would be found to contribute considerably towards a correction of the evil, even if no very great improvement should be made in the welding.

Fig. 5.

Many of our railroads have rails of the section, (fig. 5,) the upper edges of whose top are dissimilar, one being boldly rounded, while the other is left comparatively sharp. Such rails elucidate my views on this point in a very satisfactory manner; for I have observed that, in every instance, after having been for some time traversed by heavy engines, the outer, or sharp edge splits off badly, while the rounded, or inner one remains comparatively uninjured.

Consequently, this section of rail is objectionable, and I hope soon to see it fall into disuse. It was originally projected to save metal in the outer lip; but I am confident that the pecuniary advantage resulting from any such saving, is far more than countervailed by the diminution of strength attending it.

Moreover, there is another disadvantage attached to this section of rail, which is, that when the inner lip becomes worn away, as it will in time, especially on the outer lines of curves, the rail cannot be turned so as to make the original outer lip answer for the inner one. This turning of the rails I have frequently seen resorted to on the sharp curves of much-used railroads, and the rail should, by all means, be made with a view to this facility.

Fig. 6.

A bridge, or inverted U, rail, with the edges well rounded, as in this figure, (6,) will, I suspect, be found to resist splitting better than any form of section in present use; inasmuch as it not only presents a great extent of area of cohesion to be overcome before splitting can take place, but affords *less leverage* for the weight of the engines to act through to overcome it. In some cases,

Fig. 7.

I have seen the T rail split in this manner, (fig. 7,) which proves that, in this section, the welding may, from want of care, be so very imperfect, that no rounding of the edges, alone, could prevent injury.

Such disruptions as this, evince very clearly, the utility of diminishing the *leverage* through which the disrupting weight may act.

The ordinary flat-bar furnishes strong evidence of the diminution of splitting arising from more perfect welding, combined with the

parallelism of the laminæ with the top and base of the rail, aided by an absence of leverage; for it will be found, on examining such of our roads as are laid in part with the flat-bar, and in part with the T rail, that unless the latter be of an unusually strong pattern, it has split much more than the flat-bar.

The foregoing remarks will, I trust, sufficiently explain my reasons for not conceiving the splitting of the rolled rails to be attributable to any defect inherent in the nature of the material itself, but to other causes, which admit of easy remedy; and that, therefore, the principal argument adduced against the rolled rail, is not a tenable one.

Before I conclude this part of our subject, I will advance one suggestion, depending on the propriety of rounding more boldly the top edges of rails, that may possibly be of service on some of our railroads, on which the pattern for the rail to be used has not yet been decided on. It is this, that while we have been gradually increasing the *weight* of our rails, we have unfortunately overlooked the important consideration, *of applying the additional metal to the place where it is most needed.*

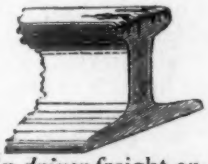
In consequence of this oversight, it will be found that some of our heavier, *improved* (?) rails, split to pieces sooner than some of the lighter ones have done, whose upper table was stouter, and better rounded.

I have had occasion to notice this more rapid deterioration of heavy rails, consequent upon want of just proportion in their parts, in more instances than one, upon very important lines of communication.

Another suggestion, having reference to the weight borne by the drivers of our engines, may not be deemed unimportant.

It is plain that, so long as we retain a top width of rail of about 2¼ inches, we cannot safely place, upon any one driving-wheel, a greater load than can be sustained without crushing, and splitting, by the ends of the rails, upon that width; otherwise the ends of the rails must be crushed, as shown in fig. 8. I shall do no more than allude to this point; my reason for introducing it is, that I feel very confident, from observation, that we have already, in some cases, exceeded the proper limit of load on a driver, warrantable by a top width of 2¼ inches.

Fig. 8.



The success which appears to attend the new *six-driver* freight-engine of that enterprising and skilful machinist, Matthias W. Baldwin, of this city, will go far towards arresting the destruction of our rolled rails. With a weight of but about two tons resting on each driver, she draws, with ease, trains weighing 200 tons over the 45 feet ascents on the Philadelphia and Columbia railroad; and the writer has seen her start, with apparent ease, a train weighing about 150 tons, up grades of 36 feet per mile, when the rails were in an unfavorable condition, being dusty, and slightly wet by a drizzling rain, which began to fall just at the time.

An increase in the number of drivers to our locomotives, which will enable us to diminish the weight borne by each, while, at the same time, it increases the total amount of adhesion, thus enabling us

to draw heavier trains with less injury to the rails and other portions of the superstructure, I look upon as the greatest desideratum demanded, at the present day, towards perfecting the railroad system.

The advocates for cast-iron rails appear to conceive that material to be free from the danger of splitting on its upper edges, found so objectionable in our present rolled rails. But this is an error; for, unless the upper edges of the cast rail, also, be well rounded, they will be found to be fully as liable to that defect as the rolled rail. The pieces split off will not, it is true, be so long as those which separate from the rolled rail, owing to the granular structure of cast-iron; but their greater number will more than compensate for their difference in length.

From the observations I have made on rails of this material, I find that the splitting, in the case of sharp-edged rails, takes place on the outer edge to a greater extent than on the inner one; and for this reason: the friction of the flanches against the inner edge of the rails has a tendency to wear that edge gradually into a curve, conformable to that which unites the flanch to the tread of the wheel; and this curve operates as a partial protection against splitting, on the principle already alluded to, when speaking of the upper edges of rolled rails.

This action is particularly observable in the outer rails of sudden curves, where the inner edge soon becomes smoothly worn to a bold curve, which acts as a complete preservative against splitting, while the outer edges of the same rails become much injured. On straight lines, the splitting under heavy engines is very serious on both edges, if they be not well rounded; but it is perceptibly greater on the outer one. I have observed the same protective tendency of the outward pressure of the flanches very sensibly exhibited also in some rolled rails which have sharp edges.

By rounding off the edges of the cast rail about as boldly as is done in our best heavy rolled rails, and at the same time diminishing the weight borne by any one driving-wheel to about 1½ tons, I think we should prevent either splitting, or very serious abrasion; and I should not hesitate to employ such a rail, under such circumstances, whenever important considerations should happen to demand it.

We will now glance rapidly at two of the principal objections urged against the cast-iron rail, viz., its brittleness, and its more rapid wear.

Admitting, of course, the former objection, the friends of the cast rail propose to obviate it by the use of continuous wooden bearings. But this expedient, although unquestionably effective so far as it removes the danger of the rails breaking, will, on calculation, be found, in many cases, to raise the cost of the cast-iron track to an equality with one of rolled iron, in which the continuous bearings are not employed; and especially will this remark apply if the bearings be Kyanized.

This consideration neutralizes, in a great measure, one of the principal arguments in favor of the cast rail, viz., its comparative cheapness.

But, in cases where it is determined either to adopt, or to dispense with, bearing timbers, whether the rail be cast, or rolled, there the proportion of expense is entirely in favor of cast-iron; even when the latter has given to it an increase of weight over that of the former, sufficient to impart to it an equality of strength.

As to the *wear* of cast-iron rails, it cannot, I think, be denied that it is much greater than that of the rolled rails. According to experiments cited in "Wood on Railroads," the comparative wear is shown to be about as 4 or 5 to 1.

He says, (see 3d London edition, page 67:)

"We shall now give some of the results of the wear of cast and wrought-iron rails, with a view of determining the depth of bearing surface.

"In the former edition of this work, we gave an account of two experiments, on the wear of cast and wrought-iron rails, upon the Stockton and Darlington railway, as follows:—*Malleable-iron rails*, 15 feet long, over which locomotive engines pass, weighing from 8 to 11 tons; wagons loaded, 4 tons each; 85,000 tons passed over in a year, exclusive of engines and empty wagons; weight of rail, 136½ pounds; loss of weight, in 12 months, 8 ounces; the breadth of the top of the rail, being 2¾ inches, gives one-tenth of a pound, per yard, per annum; and Mr. Story informs us that subsequent experiments furnish nearly the same results. In determining the premium for the best form of rail, for the London and Birmingham Railway Company, with Professor Barlow and Mr. Rastrick, we found the annual wear, estimated by some of the competitors, at one-sixth of a pound, per yard, per annum. Upon the Killingworth railway, I have had some of the rails, which were weighed and laid down in 1825, taken up and re-weighed; and find the average loss of weight of several rails, to have been eight pounds, for each 15-feet rail, in 12 years, which gives about one-eighth of a pound, per yard, per annum. These rails were laid down at a time when the manufacture of malleable-iron rails was not so well understood as at present; and, on examination, I found part of the loss of weight was attributable to exfoliation on the sides. About 100,000 tons of coals would pass over these rails annually, exclusive of the weight of the engines and empty carriages. Mr. Dixon, the resident engineer upon the Liverpool and Manchester railway, states the wear of the rails upon that railway to be one-tenth of a pound, per yard, per annum; which was determined by taking up three rails, cleaning and weighing them, and then, at the end of twelve months, taking them up again, cleaning and weighing them as before; and this being repeated for two years, the wear was found to be the same.

"We may, therefore, take the wear of the rails to be about one-tenth of a pound, per yard, per annum, which, supposing the whole to result from the wear on the upper surface, will be one-eighty-fourth part of an inch; if the top, or wearing part, of the rail were, therefore, an inch in depth, the rail would wear eighty-four years. The whole of the wear above alluded to does not, however take place

upon the top; a part, though, probably, a very small portion, is attributable to exfoliation by the action of the air: supposing, however, that the wear by the action of the wheels amounts to one-tenth of a pound, per yard, per annum; if the top, or bearing part, of the rail be made an inch in depth, it will be sufficient for all the purposes required. Any increased depth and weight, which would not be required for above eighty years, would, at compound interest, at the end of that period, amount to a greater sum than it would be convenient to expend for such a purpose, considering the remote period at which it becomes useful."

Again: he says, (page 131)

"Experiments are going on at present, where both kinds of rails, accurately weighed, are laid down, and subjected to the passage of the same quantity of traffic over them; the result of these, so far as they have gone, is in favor of wrought-iron. In the operation of making the cast-iron rails, the surface is partially case-hardened in the casting; this may be seen in all cast-iron rails, extending to a certain depth from the surface. Any experiment, showing the comparative wear, must, therefore, be continued until after the outer hardened surface be worn through; and it is presumed that sufficient time has not yet elapsed to furnish this. We have, therefore, been obliged to reject the data founded on this mode of experimenting, and shall give the result of a different sort of test, more severe, and which, it is trusted, will be deemed sufficiently approximate to justify its presentation to the reader.

"Upon the Killingworth railway we had originally cast-iron wheels upon the locomotive engines; about four years ago, we adopted wrought-iron tires. Now, as we have, in this way, the relative wear of cast and wrought iron upon the wheels which run upon the rails; and as the nature of the action will operate nearly alike, whether upon the surface of the rails or of the wheels, we shall, by that means, have a pretty near approximation to the relative wear upon the rails. In this way, we have a considerably more severe test; as, if we take the quantity of traffic, equal to 2000 tons, passing along the railway daily, and suppose the carriages to convey three tons each, with 3-ft. wheels, the relative wear of the wheels and rails is as 53 : 1, nearly.

"The average wear of the cast-iron wheels was above half an inch in nine months; and, with the wrought-iron tire, the wear of one pair of wheels has been a quarter of an inch in three years, and, with three other engines, one-eighth of an inch in twelve months; making the wear, at least, as five to one in favor of wrought-iron. The actual wear of the rails will not be to the same extent as this, as the engine wheels sometimes slip round, or slide upon the rails, in bad weather. The wear of the wheels of the common carriages will not be so much, for the same reasons; but, although it should be observed, that, from this, we ought not to deduce the actual duration of wrought-iron rails, as, their surfaces being narrower than the wheels, the wear will be, perhaps, more than proportionably greater, yet the relative

wear should, however, remain the same. We now give the following experiment made on the Stockton and Darlington railway:

"Cast-iron rails, 4 feet long, over which wagons only pass, weighing four tons each, when loaded; 86,000 tons passed over in a year, exclusive of wagons; weight of rail, 63 lbs.; loss of weight in twelve months, 8 oz. The loss of weight in malleable-iron rails was, in the same period, 8 ounces for 15 feet length; the same quantity of goods, 86,000 tons. This will give the difference of wear 15 : 4 in favor of wrought-iron rails.

"These experiments show that, if rails of the proper degree of strength be used, the durability is decidedly in favor of wrought-iron rails; and we have before observed, that, in rails properly manufactured, none of the exfoliation, or oxidation, originally dreaded, exists."

And again, in his appendix, (page 729,) he gives the following experiment:

"On the 10th of May, 1831, on the Liverpool line, a malleable-iron rail, 15 feet long, carefully cleaned, and weighing 177 lbs. 10½ oz., was laid down. On the 10th of February, 1833, the same rail was taken up by Mr. J. Locke, then resident engineer on the line, and well cleaned as before, and weighed 176 lbs. 8 oz. It had consequently lost, in twenty-one months, a weight of 18½ oz. The number of gross tons that had passed on the rail, during that time, was estimated at 600,000. Thus we see that with so considerable a tonnage, and with the velocity of the motion on that railway, the annual loss of the rail was only $\frac{1}{768}$ of its primitive weight; so that it would require more than a hundred years to reduce it to the half of its present strength."

In the United States no experiments have, I believe, been made to determine the comparative wear of the two kinds of rails; but, so far as my personal observation extends, I should have assumed a proportion at least as great as that deduced from experiments by Mr. Wood.

Having shown what authority may be adduced in proof of the statement that cast-iron rails wear much more rapidly than rolled ones, (a fact which some of the advocates for the cast rail refuse to admit,) I shall close this paper without entering into any calculations of the comparative outlays attending the wear of the two materials. These, any engineer may make for himself in a few minutes, having the foregoing data, and the cost of cast and rolled iron at the time and place where they are required to be used.

I have been induced hastily to throw together these few remarks on this important subject, in order to rectify an erroneous impression that has arisen from my recommendation of a cast-iron rail in one particular instance, that, therefore, I considered it preferable to the rolled rail in all cases.

Memoir upon the Stability of Revetments, and of their Foundations. *By* M. PONCELET, *Chef de Bataillon du Génie.* *Translated from "No.* 13 *du Mémorial de l'Officier du Génie," by Captain* JOHN SANDERS, *Corps of Engineers.*

[CONTINUED FROM VOL. V, PAGE 371.]

Upon the origin of the practical rules for calculating the thickness of retaining walls.

84. The year 1687 is the date usually assigned to the official transmission of the general profile of Vauban to the various fortresses, though it is quite probable that it was some years sooner. It was only four years later, in 1691, that Bullet, in his *Traité d'architecture pratique*, proposed a rule, not based upon the results of experience, but upon a mechanical theory, which, although very imperfect, nevertheless appears, from the celebrity of the author, and the great number of editions of his book, to have been adopted by many architects and constructors. This rule is equivalent to taking the thickness of vertical terrace walls about equal to the 0.35 of the height, and is based upon the supposition of the earth and masonry being of the same density.

85. In a manuscript memoir of 1716, dated at Metz, and which is to be found in the *Dépot des fortifications*, Buchotte, a military engineer, supposes the natural slope of earth to be 45°, and its specific gravity to be equal to two-thirds of that of masonry; which leads him, by a train of reasoning confined solely to the hypothesis of sliding, and of a coefficient of friction equal to unity, to make the weight of the wall, which was supposed vertical, equal to two-thirds of that of a prism which answers to the natural slope of the earth; or, which amounts to about the same thing, to make the thickness of this wall equal to one-third of its height. Querlongue, in another manuscript note of 1743, has, nevertheless, observed that this rule (of one-third of the height) was only applicable to *mean* earth and masonry, and that the proportion should change with the ratio of the densities. This, however, did not hinder several following engineers, and Gauthey among the number, who, in 1785, undertook to establish the uniform correctness and generality of this same rule.

86. From the estimation justly given to his other works, and by the results of the experiments with which he has accompanied his theoretical views in a memoir published at the time designated, Gauthey has especially contributed to cause this rule to be considered as one of universal application, whether as to the effects of sliding, or as to those of rotation. For if, on the one hand, he takes the density of the earth equal to four-fifths of that of the masonry, which may be considered as a limit; on the other hand, he admits in principle, from the result of experiments erroneously interpreted, that the thrust from the pressure of the earth is independent of the angle of its natural slope, and is nearly equal to the fourth, or third, of the weight of the prism at 45°. which we may look upon as establishing a sort of com-

pensation of errors. But, to show how very erroneous similar results, formed into practical rules, must be; and, at the same time, to undeceive those engineers who still oppose the adoption of the new theories which emanate from the learned researches of Coulomb, it will suffice to remark, that, according to the spirit of the empirical rules under consideration, the resistance of a vertical wall, where the retained earth was not embanked above its top, would only exceed by a tenth, or would even simply equal, the action of the pressure of the earth—evidently an absurd consequence.

What it is here essential to be established, is—1°. That the rule of the thickness being equal to one-third of the height, originated in imperfect theories, and not from a clear and judicious experience;—2°. That it is defective; and, above all, errs in this, that it overlooks the actually given quantities on which a correct solution of the question must depend (24).

87. We have not referred to Belidor, because his method, although founded upon a more advanced theory, but equally questionable in principle, has never been generally adopted as a practical rule by engineers. But, as to that of Vauban, relative to revetments, or to demi-revetments, the opinion that it was principally founded upon military and practical considerations, without reference to any mechanical theory, being quite generally entertained; we think that we should here establish the contrary, and, at the same time, show what are the principles which might have guided the author in the discovery of his rule, which is often criticised, but seldom deviated from in practice.

Vauban, in his *Traité de la Defénse des Places*, after treating of the various masonry revetments, adds: "Moreover, we should not expect a great resistance from these revetments, for they are not made with the view of resisting cannon for any length of time, as many seem to imagine, but to sustain the rampart, and to prevent an escalade, since it is certain that, if a battery of eight or ten pieces is placed upon the parapet of a covered way, with the design of making a breach in the face of the opposite bastion, and if it is well served, then, in less than forty-eight hours, it would open this face to its foundation, and would penetrate to the earth beyond it, and, however solid the revetment might be, it would overturn it," &c.

It is thus very clear that Vauban attached no military importance to the use of counterforts; and this result, derived from a long experience in sieges, has, since then, been fully confirmed by the recent experiments made at Metz, under the direction of Captain Piobert, of the artillery. A summary of these experiments will be found in his *Traité d'Artillerie théorique et pratique.* Indeed, we see, from them, if the counterforts should remain standing, by being in part engaged in the earth, after the fall of the wall, and should thus contribute to retain the earth, that it would only require some volleys of shot and shell to overthrow this earth entirely, and thereby to lessen the slope so as to render it practicable for the assaulting party. This fact, at the same time, proves that profiles, with long counterforts,

and with relieving arches, either horizontal or vertical, do not possess the military advantages that writers have erroneously attributed to them, from the commencement of the last century.

But Vauban is still more explicit in the instructions with which he accompanied his general profile, wherein he says: "This profile is accommodated to various heights of walls, is proportioned with reference to the weight of earth which they have to retain, and has been tested in the building of more than four million cubic yards (500,000 *toises cubes*) of masonry, at 150 different fortresses:" a phrase which evidently proves that this same profile was not simply the result of a long practice, but rather the consequence of rules and principles which must have preceded its application in military constructions.

Finally, the last article of these instructions being as follows, "these profiles are only proposed for masonry which has to sustain great weights of newly embanked earth, and not for that which is built against the face of an excavation where the earth has never been moved, which is generally the case with the revetments of ditches of fortifications," &c., we should conclude from it, that, under such circumstances, Vauban found the thickness furnished by his general rule to be too great. But this will not suffice to explain the motives which led him to reduce to four, or even to three, feet, the thickness at the top of counterscarps and gorge walls, in the place of five feet, which is given by the general profile, any more than it will, the reduction of the exterior slope to one-sixth. We must conclude, that Vauban not only had confidence in the solidity of his general profile, but that he knew how to introduce a judicious economy in constructions, when either the localities, or given quantities of the question, would authorize it.

88. With regard to the criticisms of this same profile by Buchotte, Couplet, Belidor, and all those who have since undertaken to investigate this question, they bore chiefly upon the apparent defect, in the proportions of the thickness and height, which makes the thickness of walls over ten metres high, too slight, and that of those under too great. This reproach appeared so much the better founded, as, in consequence of the false hypotheses then admitted upon the value of the pressure, and of the compensation of the errors which took place in the calculations; they obtained, as we have seen (85 and 86), proportions which corresponded with those of the general profile, for walls of the mean height of ten metres, and which they very naturally attributed to their having got into conditions bordering on a strict equilibrium. The fact is, that when we take at all into consideration the weight of the parapet, the influence of which decreases as the height increases, and also the effect of the counterforts, which, on the contrary, increases very rapidly with this height, we never arrive at any marked difference in the thickness. Besides, we ought not to be astonished that the rule of Vauban should only have received such slight changes, notwithstanding the numerous attacks of which it has been the object, as we shall show that they were not based upon correct mechanical principles.

Foundation of the rule of Vauban.

89. In examining attentively the constitution of this profile for the case of ordinary parapets, we perceive distinctly therein an intention to make the thickness of the masonry, following the theory of the wedge, or inclined plane, increase proportionally to the height of the retained embankment, which is composed of a variable part equal to that of the revetment, and of a constant part depending upon the height of the parapet. Only that Vauban, instead of taking the height of the parapet simply equal to 2 or 2½ metres, has attributed a much greater value to it, doubtless with the intention of securing to the revetment an excess of stability, or thickness, independent of its proper height, and sufficient to guard against various accidents, such as temporary or permanent additional loads on the ramparts, scaling of the face of the wall, penetration of projectiles, &c.; circumstances which do not in fact change with the height of the revetment. Such an hypothesis seems very rational to us, and more conformable to the true laws of physics, than that by which some engineers of our day give an excess of stability, or thickness, to retaining walls, which, in place of being constant, increases proportionally with the height, as if the causes of destruction ought themselves to increase in this proportion.

90. At all events, it is very worthy of remark, that the idea of a constant additional load appeared natural enough for Couplet to adopt it in the "*Mémoires de l'Académie des Sciences*," of 1727, as proper to secure the necessary excess of stability to revetments; and that an anonymous author, in an unpublished memoir among the papers in the Dépôt of Fortifications, should conceive that this constant additional load might really have been the fundamental principle of the profile of Vauban. But the complicated calculations and imperfect theories of these authors, were not such as to set the matter in its true light, or to cause the consequences of it to be admitted by engineers. This principle will, on the contrary, appear nearly incontestible, at least with regard to the body of the masonry, if we compare the practical formula of numbers 23 and 38, with this, $e = 0.18\,H + 1.64$ ms., which likewise represents (74), on the hypothesis of rotation, the thickness of vertical walls of the same stability with full revetment of Vauban.

In fact, if, in order to bring it to the case of strict equilibrium, we should divide the factor, 0.85, of the first of these formulas, by 1.459, the square root of the coefficient of stability 2.13, which enters implicitly into it (23); and should, moreover, suppose $f = 1$, or $\alpha = 45°$, $p = \frac{2}{3}\,p'$ hypotheses universally adopted in the time of Vauban, it becomes

$$e = 0.2\,H + 0.2\,h,$$

and gives results not perceptibly differing from those of the preceding, when we suppose that h is equal to 8.2 metres for the lowest walls, and to 7.4 metres only for the higher revetments, such as 19 or 20 metres, for example; which is equivalent to taking a mean term, h

= 7.8 metres, or, (in subtracting two metres for the height of the parapet,) to giving a constant additional load of earth 5.8 metres high, which corresponds to an additional thickness of masonry 0.2×5.8 m. = 1.16 m. This we should (according to the assumed views of Vauban) consider as necessary to secure to the body of the masonry a proper excess of stability independent of its height.

91. What is most singular in this similarity of results, is, that the value $h = 7.8$ m., coincides almost rigorously with that of the point of intersection of the inner and outer faces of an ordinary scarp; and that the same coincidence, also, very nearly occurs in the case of demi-revetments, when we compare the approximate formula $e = 0.2\ (H + h + 5.8$ m.) with this, $e = 0.18\ H + 0.202\ h + 1.24$ m., which is obtained (74) from the transformation of the general profile of Vauban.

It is evident that such coincidences are not the result of mere chance, and that they justify the assertion, not only that Vauban determined the thickness of his revetments on the hypothesis of a constant additional load of earth of about 5.8 metres in height, but, besides, that he had recognized, through the range of ordinary practice, the correctness of the rule which makes the thickness of vertical walls, with a parapet covering their summit, sensibly proportional to the entire height of the earth to be sustained. However, as we have only, so far, had under consideration the body of the masonry of the general profile, it remains for us to show that the same things occur when we introduce the action of the counterforts in the calculation of the moment of stability; for that purpose we shall assume, with all the old engineers, that these counterforts turn in a common mass with the wall, around the outer edge of its base.

92. The extent back of the counterforts from the wall being, for ordinary revetments, expressed by $0.2\ H + 0.65$ m., and their width at their junction with the wall by $0.1\ H + 0.65$ m., which gives for that at the end $\frac{2}{3}\ (0.1\ H + 0.65$ m.); we find, for the general expression of the required moment, reduced to a running metre of the profile, and in supposing the thickness at the cordon to be about 1.625 metres, and that the counterforts are distributed along at distances from centre to centre:

1° of 5 metres apart, (a') $0.000953\ H^4 + 0.029296\ H^3 + 0.40833\ H^2 + 1.456\ H$;

2° of 6 metres apart, (b') $0.000794\ H^4 + 0.026638\ H^3 + 0.39444\ H^2 + 1.433\ H$;

formulas in which the term in H^4, always very small with reference to the sum of the others, as long as H is less than 12 metres, is introduced because, on the one hand, Vauban has been obliged to make the width of the counterforts increase with their height, so as to secure them against rupture by a resistance which, in itself, increases with their weight; and, on the other hand, he wished to preserve an invariable distance between the axes of these counterforts.

If we should now likewise seek the moment of a vertical wall of the same height H, and of a thickness which may, in general, be ex-

pressed by the formula $e = k\,(H+h)$, k being a number to be determined, and h continuing to represent the height of the parapet, augmented by its load of stability, we shall obtain for its expression,

$$\tfrac{1}{2}\,e^2\,H = \tfrac{1}{2}\,k^2\,(H+h)^2\,H = \tfrac{1}{2}\,k^2\,H^3 + k^2\,h\,H^2 + \tfrac{1}{2}\,k^2\,h^2\,H,$$

in which the term in H^4 is alone wanting.

We should not expect that the numerical coefficient of the formula $e = 0.2\,(H+h)$ of number 90, would satisfy the condition, since it only refers to the stability, itself, of the mass of the masonry; therefore, in making $k = 0.2$ and $h = 7.8$ m., we obtain only, for the corresponding value of the moment,

$$0.02\,H^3 + 0.32\,H^2 + 1.28\,H.$$

But, on the contrary, we shall make the results almost rigorously coincide, by taking $k = 0.2357$, $h = 7.2$ metres; for we shall then obtain the moment,

$$(c') \qquad 0.027798\,H^3 + 0.40000\,H^2 + 1.440\,H,$$

which is about a mean between those in question, if we omit all consideration of the term in H^4.

93. It is, besides, easy to perceive why we do not again fall upon the height of the load, $h = 7.8$ m., of the preceding case; for the solidity of the connexion, and the economy in the expenditure, having brought about the giving to counterforts a thickness which increases with the height, it has also been necessary, as is shown by the drawings of the *tracé* of the general profile, that the planes passing through the edges of their bases should converge at less heights above the summit, than that which belongs to the scarp itself, so as to lessen the influence of the term in H^4, which, in the expression of the moments, arises from this increase of thickness.

Nevertheless, if this explanation is not satisfactory, and it is desired that we should confine ourselves to the values $h = 7.8$ metres, and $k = 0.2357$, we shall find for the moment of a vertical wall, whose thickness $e = 0.2357\,(H + 7.8\text{ m.})$,

$$(d') \qquad 0.027778\,H^3 + 0.433337\,H^2 + 1.690\,H;$$

and then it will also be necessary to admit that it entered into the views of Vauban to correct, in part, and for the mean height $H = 10$ metres, the influence of the term in H^4. We actually find, on this hypothesis, for the value of this last expression 88.00, and for those of (a') and (b'), to which we compare it: 94.22 and 83.35 respectively.

Whichever we may adopt, the consequences to be drawn from the result remain nearly the same, with regard to the principles which must have guided Vauban in the composition of his profile for ordinary revetments.

94. As to the rule of demi-revetments, it is very naturally decided on by this consideration, that it will only be necessary in the expressions $E = 0.2\,H + 1.625$ metres for the thickness at the base of this profile, and in $0.1\,H + 0.65$ m., $0.2\,H + 0.65$ m., &c., for the horizontal

dimensions of its counterforts, to replace H by $H+h-2$ m., in order to obtain the corresponding dimensions of a demi-revetment of any height H whatever, surmounted by a parapet of earth, of a mean and effective height h above the cordon. For, in likewise making this substitution in the analytical expressions of the moments (a'), (b') and (c'), or (d'), which renders them applicable to demi-revetments with the outer face either inclined, or vertical, they will continue to correspond together to the same degree of approximation. A result evident in itself, as soon as we admit, for vertical demi-revetments, the approximate rule

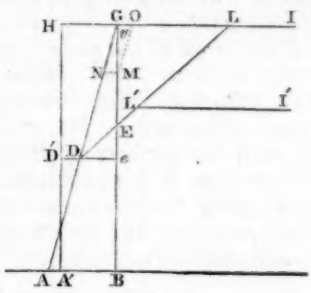
$$(e') \qquad e=0.2357\ (H+h+5.5\text{ m.}),$$

5.5 m. here representing the mean additional load requisite for stability.

These approximations would be sufficient to prove, rigorously, that the rule of Vauban belongs to a more advanced mechanical theory than that of the authors who have endeavored to criticise it. But I shall endeavor to put it in a still clearer light, and to trace even the train of ideas by which Vauban might have arrived at it.

Principles admitted by Vauban in the establishment of his general profile, and their consequences.

95. Let A'BGH be a vertical wall retaining the terrace BGI, on a level with its summit; from the principle of the wedge, or inclined plane, the pressure upon any height GM whatever, or the horizontal effort necessary to retain the weight of the prism of earth GMO upon the slope OM, which is at any angle v whatever from a vertical, will be, in neglecting friction,



$$p \times \text{GMO}\ \frac{\text{GO}}{\text{GM}} = \tfrac{1}{2}p + \overline{\text{GO}}^2 = \tfrac{1}{2}p\ \text{tang.}^2\ v \times \overline{\text{GM}}^2;$$

which demonstrates that the pressure increases, as in liquids, proportionally to the square of the height GM, and that all the difference is simply in the factor tang.^{2} v, which, in liquids, becomes unity.

This single consideration was enough to satisfy Vauban, 1°, That the centre of the pressure is at about one-third of the height GM of the prism, measured from the point M; 2°, That the triangular form ABG of the profile of masonry, is that which secures the equilibrium of every part GMN, whether for the case of rotation, or of that of sliding; 3° and lastly, That the angle GMO or v of the prism of pressure, could be neither zero, which is evident, nor an angle of 45°, that assumed as the natural slope of mean earth, since it would reproduce the hypothesis of fluidity, for which likewise tang. $v=1$, but rather an

intermediate angle, a half for example, or such that $GO = \frac{1}{2} GM$, tang. $v = \frac{1}{2}$; which Belidor, as far back as 1729, trusting to reason and the results of experience, desired to make it.

According to that, if we consider the equilibrium of any triangle ABG of masonry whatever, under the action of the pressure $\frac{1}{2} p$ tang.$^2 v \times \overline{BG}^2$, acting against the whole extent of its height, we shall especially obtain, on the hypothesis of rotation,

$$p' \tfrac{1}{2} AB \times BG \times \tfrac{2}{3} AB = \tfrac{1}{2} p \text{ tang.}^2 v \times \overline{BG}^2 \times \tfrac{1}{3} BG;$$

from whence

$$AB = \text{tang. } v \sqrt{\tfrac{1}{2} \frac{p}{p'}} \times BG.$$

Taking, with Belidor, tang. $v = \frac{1}{2}$, instead of $v = \frac{1}{2}$ 45°, and tang. $v = 0.4142$, according to the latest and truest theories; and making moreover, $p = \frac{2}{3} p'$; this equation will give $AB = \frac{1}{2} \sqrt{\frac{1}{3}} BG = 0.2887$ BG, in the place of $AB = 0.239$ BG, which we would obtain from the second value of tang. v.

96. The first of these solutions giving to the wall an outer slope of nearly $\frac{3}{10}$ upon a vertical, Vauban ought not to have admitted it, although the constructions of his time offer examples of scarps with slopes of $\frac{1}{4}$; and in limiting himself to that of $\frac{2}{10}$, he has sacrificed a part of the advantages inherent to the profile of equal resistance; advantages which he has, however, regained by adopting a system of counterforts, properly distributed in order to supply that which was wanting in the moment of the wall.

Observing, now, that the ordinary form of the superincumbent mass of earth sustained by a revetment, such as ABCD, for example, is not CGI, but DELI, which takes away the pressure from along all the height GE, and renders the part above the cordon CD entirely superfluous for retaining the earth: we might suppose that Vauban, in comparing the moment of the prism of masonry CDG, with reference to the point A, to the sum of the moments of the pressure against GE, and of the prism of earth CDE, must have allowed them to be nearly equal, which comes to about the same thing as regarding the mass of masonry ABCD as being virtually in a state of equilibrium under the action of the mass of earth BELI. We could readily explain, on this hypothesis, and on that of a constant additional load for stability, which we shall represent, for example, by I'L'LI, the general disposition of the profile of ordinary revetments and demi-revetments. But a comparison between the moment of the wall ABCD, and that of this profile with its counterforts, does in no manner sustain such a suspicion; and we are forced to return to the consideration of a vertical wall, backed in with earth to a level with its summit, to discover the real motives and actual point of departure for the rule of Vauban.

97. Then let A'BGH be a rectangular profile of masonry, subjected to the action of the pressure of earth along its whole height BG, we shall have, for the condition of equilibrium,

$\frac{1}{2} p' \overline{A'B}^2 \times BG = \frac{1}{2} p \tan.^2 v \times \overline{BG}^3$; or, $A'B = \tan. v \sqrt{\frac{p}{3p'}}\, BG$;

and if we suppose, as heretofore, tang. $v = \frac{1}{2}$, $p = \frac{2}{3} p'$, we shall find $A'B = 0.2357\ BG$, which gives,

$$e = 0.2357\ (H + h)$$

h designating the total height CG of the superincumbent mass of earth. For the case of sliding upon the base of the wall, we should have, (f' being the coefficient of friction,)

$$f' p' \times A'B \times BG = \frac{1}{2} p, \tan.^2 v \times \overline{BG}^2, \text{or } A'B = \frac{p}{2 f' p'} \tan.^2 v \times BG.$$

In taking, as was done in the time of Vauban, $f' = \frac{1}{3}$, $p = \frac{2}{3} p'$, tang. $v = \frac{1}{2}$; this formula coincides very nearly with the preceding one. Thus, in this method, the equilibrium would also be secure on the hypothesis of the wall sliding upon its foundation.

Now, it will be observed that the former formula, when we replace h in it by ($h + 5.5$ metres), coincides exactly with (e') the one obtained in number 94, for the thickness of vertical revetments of the height H, which have the same stability as that of Vauban with counterforts. We can, therefore, with propriety, conclude that he had discovered, by geometrical trials made directly upon the given quantities of the problem, the kind of compensation of errors which takes place in such cases; that is to say, for a vertical revetment A'BCD', retaining a superincumbent load DLI; this compensation is between the moment of the mass of masonry CGHD', which is thrown out, and that of the triangle of earth CDE, (the weight of which is added to the wall A'BCD',) together with that which would arise from the diminution of the pressure against the whole or primitive height BG, at least as long as CG does not attain twice the height BC.

98. Although the verification of this result may be rather delicate, particularly in regard to the determination of the length of the arm of the lever of pressure, it is not a sufficient motive for believing that Vauban did not arrive at it, because this determination depends only upon the most elementary geometry. If any one doubts it, it will suffice to read the writings of the old authors, such as Belidor, who have endeavored to investigate this question in the case of loads having the form of a parapet. It will not, perhaps, be useless to remark, on this subject, that, if we adopt for the measure of the pressure of earth against a rectangular revetment A'BCD' (fig. 5), the expression $\frac{1}{2} p \tan.^2 v \times \overline{BG}^2$, referred to the total height of the earth to be retained, and take for the reduced length of the arm of the lever of pressure, one-third of the height BC only, we would again exactly fall upon the formula $A'B = 0.236\ BG$; because that would, as above, give the equation of equilibrium, $p \times \frac{1}{2} \overline{A'B}^2 \times BC = \frac{1}{2} p \tan.^2 v \overline{BG}^2 \times \frac{1}{3} BC$, or $A'B = \tan. v \sqrt{\frac{p}{3p'}}\, BG$. Vauban might easily have dis-

covered that the necessary compensation takes place in the errors arising from these two hypotheses.

99. Besides, whatever may be the opinion entertained of this explanation, it will none the less be proved, that, on the hypotheses generally assumed by old engineers (75) upon the function of the counterforts, the stability of the profile of Vauban is found, as far as regards rotation, to be essentially equal to that of a vertical wall of the same height H, and with a thickness given by formula (e') of no. 94. Now, this furnishes at once (72) the means of transforming this profile into an equivalent one without counterforts, and with any exterior slope whatever.

In those cases where the explanation is adopted, we must conclude from it, that, if the rule of Vauban errs in any point, it evidently does not in theory, but rather from its attributing to tang. v, or to k (92), values 0.50 and 0.236, a little too great, which it would be more exact to replace by about 0.414 and 0.2; nor, moreover, from increasing the thickness in a proportion so much the more, as the height of the superincumbent mass ($h+5.5$ m.) is greater with reference to that of the wall; but really from depending on the hypothesis of *mean* earth and masonry.

100. Besides, if, while admitting the principle of a constant additional load for stability, in height equal to 5.5 metres, we should wish to generalize the rule of Vauban, nearly as he would himself have done it, it will suffice (97) to replace the coefficient 0.236, by the expression

$$\tfrac{1}{2} \text{ tang. } a \sqrt{\frac{p}{3p'}}$$

in which we should also, for greater exactness, and in conformity with the theory of Coulomb, put tang. $\frac{1}{2}\, a$ in the place of $\frac{1}{2}$ tang. a.

Then we should take

$$e = \text{tang. } \tfrac{1}{2}\, a \sqrt{\frac{p}{3p'}}\,(\text{H}+h+5.5 \text{ metres}),$$

for the thickness of vertical revetments, or the thickness (72) at one-ninth of its height of a revetment, without counterforts, of the same stability, but with its outer face inclined.

But, in making use of this formula, we are to recollect, that, independently of the hypothesis of a constant additional load of 5.5 metres for stability, which, even according to the remark of Vauban (87), would not answer for counterscarps and gorge walls of military works, it will still lead (21 and 35) to an excess of thickness in the case of parapets preceded by wide berms, and in that of low demi-revetments retaining very high embankments. In such circumstances, it will tend to economy, while fully maintaining the principle of stability of Vauban, to calculate the thickness by means of equations (s), (t), (u), and (s') of the 26th and following numbers, in which we must substitute 1 for δ and ($h+5.5$ metres) for h.

Mr. Vignoles' Lectures on Civil Engineering, at the London University College.

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SECOND COURSE.—LECTURE VIII. ON CURVES.

In continution of the subject of curves, Mr. Vignoles explained that in many cases it was impracticable, or inconvenient, to apply, on particular ground, the approximate rule given in the last lecture, of setting out curves chain by chain, or other short lengths, making each the side of a regular polygon, the set-off being constant. In that method the given length was strictly a secant, and not a tangent, to the curve. Another formula was more generally applicable, and sharp curves on hill sides, through thick woods, had been quickly and accurately set on therefrom. It was this: $offsets = radius - (radius - tangent)^{\frac{1}{2}}$; the demonstration of this was given, and illustrated by a diagram. For the field, tables calculated beforehand, for the greatest number of usual curves, should be prepared; but, on the occurrence of any peculiar cases, the calculation could be very readily made, with the help of a pocket table of natural sines. The Professor then recapitulated some of the leading points that had been gone over in detail at the last lecture, observing that, on the three principal expedients for counteracting the injurious effects of curves, the usual measurements might be easily remembered, viz., half an inch for the "cone" of the tread of the wheel; one inch as a *maximum* amount of "play" of the wheels between the rails, (it being disadvantageous to allow too much play;) and one inch for the extreme elevation of the outer rail in laying the way, that being the measure due to a velocity of 25 miles an hour, on a curve of half a mile radius. Mr Vignoles then observed that the "cone" being given to the wheels on account of the curves, when the line of road was perfectly straight, this conical formation of the tyre was not required, and the general disadvantage of such a form of wheel, not bearing upon the whole face, or upper button, of the rail, preponderated. It had, therefore, become customary to incline the rail, to meet the cone of the wheel, and this should always be done, both on straight lines, and on curves whose radii are not small. This inclination of the surface of the rail is obtained by casting the receiving chair, accordingly; on rails, having a continuous bearing on longitudinal sleepers, or bearing direct on cross timbers, without the intervention of chairs, the wood is cut to the requisite angle; or the inclination is sometimes given to the rails in passing through the rolls. Without this precaution of inclining the bearing surface of the rail to meet the cone of the wheel, the edge rapidly wears, and the laminæ of iron peel off in strips, more or less, according to its quality; and there is no more critical test of the perfection of rolled iron rails, than the manner in which the button edges go through this ordeal. With the above precautions of "cone," "play," and the elevation of the outer rail, the resistances opposed by curves to a single carriage may be considered to be annihilated; but

when the trains become very long, there must, of necessity, be a considerable lateral action and grinding, from the change of direction of the original drawing force through a number of carriages; but Mr. Vignoles stated that, although no conclusive experiments had been made to show the exact amount of resistance from this cause, his own observations and experience led him to conclude that the degree of curvature on railways might be safely extended further than they have hitherto been laid down on principal lines.

Mr. Vignoles referred to former observations of his, that the public would be better accommodated by more frequent departures of smaller trains, and that with such trains the curves would be of still less importance, adding that it could only be by the introduction of greater curvatures to save expense; and, as he had repeatedly argued for the same reason, by the adoption of steeper gradients, that the benefits of railway communication could be extended through many districts, and to the more distant parts of the country, as on the most economical principles of construction. Mr. Vignoles referred to the report of the Irish Railway Commissioners, and to the works of Mr. Wood, M. de Pambour, Lieut. Lecount, and other writers, for further details on curves, observing, in conclusion of this part of the subject, that where curves are so quick as to require it, especially in crossings, the additional precaution of guard rails becomes expedient.

In adding a few words on the subject of coupling carriages together in a train, the Professor insisted strongly on the draw-boys being always in the centre, and observed that, as a general rule, the connecting links should be screwed up as stiff as possible consistently with the curves of the railway, as, otherwise, the carriages are apt to swing. He mentioned that the best coupling was that of Mr. Henry Booth, the talented manager of the Liverpool and Manchester railway from its very first origin. But, as a general form of combining the draw-boys and buffers on a central rod, or tube, with spiral springs acting solely from the centre, Mr. Vignoles spoke in the strongest terms of the apparatus of Mr. Thomas F. Bergin, the manager of the Dublin and Kingstown railway, on which line they had been used with advantage for a number of years.

LECTURE IX.—ON TUNNELS.

In proceeding to treat of the subject, which might be termed that of the great works of art, to be introduced in the formation of roads, or canals, but particularly of railways, Mr. Vignoles said that it would not be possible in the lecture room to go into the details of the construction, but that he must limit himself to general principles. The rules for consideration when such works ought to be adopted, were sufficiently simple; for example, to determine where tunnels should be substituted for open cuttings, or viaducts for embankments. The French engineers, who are in general very much better mathematicians than we are, and, probably, from that very circumstance, more inclined to be theoretical, are much in the habit of introducing formulæ, which, often very useful, are not always readily applied by the practical men of this country. Supposing it to be required to deter-

mine at what point on a longitudinal section, (for road, railway, or canal,) it is advisable to begin to tunnel, instead of continuing a simple excavation—that is, the point where it becomes as cheap to tunnel as to cut open; for such a case, the following formula is given by an eminent French engineer: Let x=depth of cutting; l=breadth of road, railway, or canal, on the traveling surface; a=depth of bed, of road, or railway, to be first excavated, and afterwards filled with road material, or ballast; or depth of canal below the water-line; $\frac{x}{m}$= slope of excavation; p=price of the cutting per cubic yard. The expense of excavation per yard forward of the sectional area of any cutting, will consequently be $a\,p \times p\,(l \times \frac{x}{m} = x$; and when this price exceeds the price per lineal yard forward of tunneling, the latter is cheaper, supposing the given prices to cover all risk and contingencies in each case. But, as circumstances are continually varying, the English engineer so repeatedly finds that he has to modify—and perhaps finally abandon—the general theoretical rule, and fall back on his own experience, and that of the contractor he may be disposed to employ, that, although he may occasionally resort to such a formula as an approximation, he ceases to employ it in practice, and obtains the sectional area of the given cutting in superficial yards, by simple mensuration, and multiplies it by the price. All the complex conditions involved by slips, faults, water, and the numerous incidental occurrences in great works, to occasion unforeseen expenses, render prices uncertain, and prevent any fixed general rule; and it is only when the materials and probable contingencies are perfectly well known, that the element of cost can be safely introduced into the mathematical formula. For dry, indurated sands, gravel, sandstone rocks, &c., calculations may be made within probable limits of error; whereas, in many instances where the theoretical rule and general opinion, even of those sufficiently experienced, would recommend tunneling, it has been tried in vain, abandoned after great expense in contending with water, and recourse had, after all, to open cutting. The average cost of tunneling, upon the principal railway lines, as actually executed, appears to be about 60*l.* per yard forward, in some instances as much as 100*l.*, especially when driven forward in reckless haste, and in attempting to sink shafts, or drive drifts, without due consideration as to the quantity of water in the various strata, or the means of at once grappling with the difficulties of drainage, or pumping. With great facilities, favorable material, and not too much hurrying, the same area of tunnel has been driven for as little as 20*l.* per yard forward. In round numbers, and on an average, the sectional area of the ordinary tunnel for a double line of railway, to be worked by locomotive engines, may be called fifty superficial yards when finished, or within the ring of brickwork, or masonry, if lining were required; in this latter case, the sectional area of the opening to be excavated may be assumed as about eighty superficial yards. Mr. Vignoles observed that, for future tunnel operations, with the benefit

of past errors and experience, by avoiding undue haste in execution, and with sufficient caution and activity, 40*l.* per yard forward for tunneling may be taken as an average approximate fair price. Now if it were wished to compare this expense of tunneling with the cost of open cutting, the Professor observed that, from his experience, which had been very considerable, in removing earth in large quantities, he was not disposed to put a less price than that of 1*s.* per cubic yard for removing material for deep excavations, especially when this price is to cover contingencies of slips, &c.; with such a price, then, an open cutting 55 ft. deep, roadway 24 ft. wide, and soil requiring slopes of two horizontal to one perpendicular, would give a sectional area of 800 yards—that is, the expense (in estimate) would be the same as that of tunneling. But Mr. Vignoles observed that, in addition, the future maintenance of the tunnel should be taken into consideration, as well as whether the material from the cutting could be disposed of with advantage; the nature of the soil, and a variety of other circumstances which he stated, all of which would influence the decision. In soft rock, which would work with facility, and yet stand nearly perpendicular, the depth might be very much greater than 55 ft. before tunneling would be cheaper. In such cases, a depth of 80 ft. and upwards had been resorted to. In chalk, the proper slope to be given, which was very variable, would greatly alter any elements of calculation; while, on the other hand, in forming tunnels through chalk, experience had shown that water was the great enemy, and had entailed enormous expenses. The Professor went into a great many other points for comparing excavations with tunneling, but they appeared too technical to be satisfactorily explained in a brief abstract such as this.

On Viaducts and Aqueducts.

Mr. Vignoles next proceeded to the consideration of viaducts and aqueducts, into which, he observed, a totally different set of conditions enter, the cost varying from 20*l.* per lineal yard to the price for which no rule could possibly be laid down. Viaducts such as that of which the London and Greenwich railway wholly consists, may, probably, be executed for 20*l.* to 30*l.* per yard, including the foundations. Of course, the foundation entered materially into the calculation, and, where water had to be crossed, largely increased the expense. In some peculiar instances, a large river viaduct, or bridge, has cost as much as 200*l.* per lineal yard. The Professor instanced a viaduct he had built over the river Ribble, at Preston, for the North Union railway. The length was 300 yards, the height about 45 feet above the water, and the whole mass, including concrete foundations (where the rock was not attained), comprised about 25,000 cubic yards; coffer-dams were used for the piers, and for one abutment. The bridge consisted of five arches, each of 120 feet span, batiring on the face and spandrils from the parapet to the impost course—roadway about 27 feet wide; the total cost, including all contingencies, was £45,000, which is 150*l.* per yard forward, or 36*s.* per cubic yard on the whole solid contents; this might be considered a low

price, inasmuch as an ordinary brick bridge, of twenty to thirty feet span only, and with facilities for construction, can seldom be built for less than 20*s.* per cubic yard. Where no water, or expensive foundations, are to be encountered, and where the spans of the brick, or stone, arches do not exceed about 60 or 70 feet, 1*s.* per cubic foot on the solid contents of the viaducts may be put as a good covering price. Mr. Vignoles stated that, for such viaducts, about 60*l.* to 70*l.* per lineal yard might probably be taken as the average approximate cost, and the additional expense, from a considerable increase of height, does not become so very great, as it chiefly affects the piers only. The Professor then enlarged much on adopting timber arches, with piers of masonry, for viaducts of large span and great height, and produced a number of drawings of such bridges, some actually constructed, and some only proposed. The heights were from 70 to 130 ft., the *minimum* price being 35*l.*, and the *maximum* 80*l.* per lineal yard. He further pointed out that high embankments should be avoided, and timber viaducts substituted, as a mere point of economy, even without taking into consideration the risk and danger of slipping in such great masses of earth. In an embankment only 40 ft. high, an occupation bridge for a farm would often cost nearly £1000; it was, therefore, only in crossing a very narrow valley or ravine, where no bridges under would be called for, and no masonry—except perhaps a culvert of the very smallest dimensions—that very high embankments should be made. Mr. Vignoles alluded to several such, varying from 70 to 90 ft. high, which he had made, and pointed out a terrible failure in one case, although in other instances success had followed. In passing through hilly countries and along mountain sides torn by ravines, the introduction of the timber-top viaduct, with stone piers, to overcome points of great but partial difficulty, was strongly recommended, especially as great additional height of viaduct could be given at small expense, and thus excavations on each side saved.

LECTURE X.—ON THE UPPER WORKS OF RAILWAYS.

This lecture was on the upper works of railways, which term, the Professor stated, was intended to comprehend every thing above the bed, or formation level, of the roadway—viz., the gravel, broken stone, or other road material, technically called the "ballasting;" the stone blocks, or wooden cross sleepers, laid thereon; the chairs and rails, and their fastenings, as attached to the stone, or timber, supports; and the boxing, or filling up, of the road material around these supports, when the railway is finally laid to the proper gauge, range, and level—the whole of the materials and adjustment forming the "upper works"—an expression borrowed from the German *oberwerke*. The depth from the road-bed to the level of the rails, of all these parts, as permanently laid together, is seldom less than two feet, when a good way is to be insured; the principle to be observed being to have the ballasting of such a material and nature that water will percolate freely through, as clean gravel, cinders, quarry rubbish, coarse sand, &c. The word "ballast" is a northern provincial term.

Some of the first railways were introduced into the vicinity of Newcastle-upon-Tyne, Shields, and the neighboring coal shipping ports, and it being found necessary to have a bed, or layer, of some material to receive the railway track, the same not to be retentive of water; the gravel brought by the colliers from London as ballast, and accumulated in hills, or spoil-banks, near the sides of the harbors, was found to answer the purpose very well; the expression has since become common for whatever other material was similarly used for laying railroads. Mr. Vignoles observed that a great number of other technical words, now in common use when treating on railway works, were provincial terms, chiefly from the north of England. When the bottom of cuttings, or top of embankments, is of soft material, it was recommended to make a kind of pavement, or hard layer, on the forming level, below the ballast. On embankments, until they became well consolidated, this pavement would not, perhaps, be conveniently put in; still, means should be contrived to carry off the water quickly from the surface; and the Professor insisted much on the free introduction of blind, or French, drains, of broken stone, among, or under, the ballast, connected with the open side drains, which should never be omitted in cuttings; on embankments, these cross rubble drains should go free of the road-bed, with water channels down the slopes, of sods, or more substantially formed if needful.

The bed of the railway having been prepared on the above principles, next came the consideration of the railway itself. The substance first used for this trackway was wood, and afterwards a metal rail plated on the wood; after 200 years' trial of different systems—for so long was it since the first colliery wagon-ways were introduced—the Professor observed, we are just coming back to the original form and material as best adapted for the purposes of a railway, viz. timber laid longitudinally. When blocks of stone are used to support the chair and rail, weight seems to be sought for as desirable, and the general cubic contents of such blocks as are considered proper for a good way are about five cubic feet, viz., 27 inches wide, and a foot thick. When cross bearers of wood are the supports, each sleeper, as it is termed, contains about two cubic feet of timber, being about seven feet long, with a transverse section of 40 square inches, being full eight inches broad, and averaging five inches thick. The intervals at which the blocks, or sleepers, are placed, vary from three to five feet, according to the weight of the rail, or of the load. With these descriptions of support, it becomes generally necessary that a chair, or saddle, be attached thereto, to receive the rail, which is seldom fastened directly either on block or sleeper, without the intervention of this contrivance of a chair, unless when the rail is made wide and flat at the base. Such shaped rail is now much used in cross bearers of wood for temporary railways, by contractors, when executing works. Mr. Vignoles said this form of wrought-iron rail was first introduced, nearly twelve years since, on the St. Helen's railway, by some contractors, at his suggestion; and the same rails were lately in use by the same persons, and still good and serviceable after continued use. Of late it had been recommended by many en-

gineers—among which Mr. Vignoles believed he was among the first—to lay down the railway bars on bearings of timber, disposed lengthways in the direction of the way, and upon which the iron had a continuous bearing, instead of having it supported at intervals (either with or without chairs), as was the case when blocks, or cross timber bearings, were used. In describing the different modes of laying rails, the Professor observed that the heavy stone blocks being packed around, or boxed up, with ballast, kept the rails in place—that the cross sleeper having both rails attached thereto, the gauge, or breadth, was preserved; with the longitudinal system of bearings, the parallelism was retained by cross ties of wood, with tenons, and sometimes by rods of iron with screw-ends and nuts, and occasionally with both. It was necessary thus to provide for preserving the breadth of the railway, for, as the carriages and engines work along the rails with a wriggling motion, there is always a tendency, by the lateral action of the flanches of the wheels, to push the road out of gauge. Mr. Vignoles mentioned the two new lines added on the south side of the London and Greenwich railway, as the latest examples of longitudinal timber bearings; but he observed that, as the great point in this system was to insure that the rail be firmly attached to the wood, to prevent any vertical play, he considered a more effectual method might have been there used, so as to have the iron continually united to the timber, on a plan which he had tried with success, and to which he would presently advert. When iron was first introduced for railways, it was for a long time merely a plating of metal on the edge of the wood rail, on which plan, with iron bars of greater or less weight, many of the lines in America had been laid. Cast-iron being next introduced, the system of fastenings was necessarily changed, and the original longitudinal timbers abandoned for cross sleepers, or isolated stone blocks. The rails being cast in lengths of three or four feet, it was found expedient to prepare some contrivances to receive and fasten the ends together; and this was the saddle, or chair. Some of these iron rails were cast deeper in the middle, and, from their shape, got to be termed "fish-bellied," this form being probably adopted with the idea of obtaining uniform strength; though, for railway purposes, the position of this increased depth was the reverse of that given to bearers intended to resist quiescent weights. From the action of the moving weight, however, upon rails with so many joints, they soon got out of order. Wrought iron was then introduced, to get greater lengths; the first of these were rolled, at a considerable expense of useless ingenuity, into the same form as the fish-bellied cast-iron rails, the length of 15 to 18 feet being divided into five or six flat ellipses. On most of the lines where this description of rails was first laid, it has been found necessary to take them up, and replace them, as they were found to break at a short distance from the points of support. Mr. Vignoles stated that he had, from the first, decidedly set his face against this form of rail, and argued for and introduced rails with the same section throughout their length—since commonly styled *parallel rails*, as distinguished from the "fish-bellied rail," adding 10 lb. to the original weight of 35 lb. to the yard, by putting on a lower rib, or

web, on the principle that such gave additional stiffness to all beams; this lower web was increased in size from time to time, until, in a special report to the London and Birmingham Railway Co., Mr. Vignoles recommended that the upper and lower webs, or buttons, should be made precisely alike, to allow the rail to be turned up or down, or in either direction. This form was, however, first actually laid down by him on the North Union railway, and its advantages in the above respects have already been appreciated and applied; these rails are about 65 lb. to the yard. The Birmingham Company decided, finally, on adopting this form, increasing the weight to 75 lb.; and, where chairs are used, it is now almost exclusively employed, the weight being sometimes increased to 78 or 80 lb. per yard. With the increase in the weight of the rail, the intervals between the supports also gradually increased from three to five feet, but with bad effect, as the expense of keeping a railway in order with the longer bearings (as the technical phrase is) was very much augmented; and, on the London and Birmingham railway, intermediate supports had since been introduced, where the original bearings were five feet. Mr. Vignoles stated it as his opinion, deduced from considerable experience and observation, that, where chairs and supports at intervals were used, he considered a 60 lb. rail, with a 3-feet bearing, better than a heavier rail with a longer bearing. Blocks, he observed, were, however, nearly exploded as supports; the cross sleepers and chairs were still preferred by many engineers; but it was certain that, the closer the supports—that is, the shorter the bearings—the less the cost of maintenance; and hence the inference, which experience every where confirmed, that the continuous supports were best of all. In respect of fastening the chair to the block, or sleeper, and the rail to the chair, it was now almost universally admitted and acted upon, that compressed wood was much preferable to iron spikes, bolts, or keys. Mr. Vignoles introduced a number of drawings, and described a variety of diagrams, illustrating the various shaped rails and chairs, and modes of fastening adopted, and drew comparisons as to the advantages and cost of each. In reference to what he had before stated of the disadvantageous method hitherto pursued in fastening rails with flat bases to continuous timber bearings, by spikes, or screws, the Professor said that such a mode seldom continued to hold the rail close down to the timber, and there ensued a certain quantity of vertical play of the rail on the wood, often accompanied with a good deal of rattling; and, in the end, the head of the bolt, or spike, was absolutely jerked off. Mr. Vignoles said that the only effective fastening was that used with Evans' patent rails, which had a slit, or groove, of a dove-tail shape, (in cross section,) rolled for the whole length of the rail in the bottom; bolts, with similar shaped heads to fit, were passed into this groove, and dropped, at the necessary intervals, through holes in the longitudinal timbers; the bolt terminated in a screw, and, a washer and nut being put on by means of a spanner, the nut drew down the rail closely to the timber. Mr. Vignoles stated that he had had a considerable length of railway thus laid, which had been done some time, and the rail had remained close

down on the wood without any play, or getting at all loose. He concluded this lecture by stating that, in the next, he would endeavor to draw a comparison between the modern heavy rail, and chair and fastenings, as used with cross timbers laid at intervals, and the rail and fastening, as above described, to be laid on longitudinal timbers, and having a continuous bearing thereon.

(To be continued.)

Major General Pasley on the recent great Mining Operations near Dover.

[The following letter on this interesting subject appeared from Major General Pasley, Inspector General of Railways, in the Times of Monday.]

Sir—Having had frequent questions put to me in conversation respecting the great explosions near Dover, by which Round-down Cliff, an immense projecting mass of chalk in the proposed line of the South-Eastern Railway, was thrown down, I request your insertion of the following statement, in order to correct several inaccuracies in my own letter to you of the 23d of January last, which I wrote in haste, that it might appear in time to remove the impression which I found generally prevailed, that the whole operation was under my direction, but which I considered only a vague report, until I saw it quoted from an article in a paper, which quotation did not come to my knowledge until three days before the time appointed for the firing of those great mines.

To Mr. W. Cubitt, the engineer in chief of the South-Eastern railway, is justly due the merit of having conceived the idea of removing a mass of chalk rock, nearly 300 feet in length, but of still greater height, and averaging 70 feet in thickness, by simultaneous explosions of gunpowder, instead of employing laborers to scarp it away, which would probably have cost £8,000; and the merit of success also belongs to him, inasmuch as he took the most judicious measures to insure it; but, as he informed me that he never would have contradicted the reports which ascribed the entire superintendence of that great operation to me, and as he is not likely to publish anything on the subject, I am desirous not only of correcting the inaccuracies in my first letter to you, but also of supplying the omissions in the printed accounts, by noticing the useful labors of those who contributed to his success, which I have always made a point of doing, in every similar operation that has taken place under my own direction, and which I am sure that Mr. Cubitt would do if he wrote himself, as I know from the able resident and assistant engineers of the same railway, that, instead of assuming the whole merit of the works in which they have been employed under him, he has always been ready to acknowledge their services in the most liberal manner, both officially to the directors of the company, and personally in conversation, as I have witnessed myself.

The general impression that the mines near Dover were to be su-

perintended by me, no doubt arose from its being known that Mr. Cubitt always intended to consult me, and that he would not, and did not, decide upon his plan of operation until after he had taken my opinion; and it was also known that he relied entirely upon my assistance for firing his mines simultaneously by the voltaic battery, of the use of which, as applied to mining, neither he nor his assistants had had any practical experience. Accordingly I went to Dover by his request, and introduced Lieutenant Hutchinson, of the Royal Engineers, who had been employed two summers under me in the operations against the wreck of the Royal George, and who happened, fortunately, to be on duty at that place at the time; so that I recommended him as the most proper person to superintend the firing of the proposed mines by the voltaic battery, provided that the permission of the Master-General of the Ordnance could be obtained to enable him to undertake this service, which was readily granted, on application being made to Sir George Murray by the railway company. Accompanied by this officer, I examined the drawings of Round-down Cliff, that had been prepared under the superintendence of Mr. John Wright, the resident engineer of this portion of the railway, to whom Mr. Cubitt referred me on the 10th of November last, and went with him into a drift, or small gallery, cut entirely through the cliff, and about 248 feet in length, which had originally been intended for the commencement of a tunnel through which the railway was to pass; a design that was abandoned afterwards on discovering that this part of the cliff was likely to give way sooner or later, and the plan of removing it by gunpowder was adopted in consequence. Shafts about 17 feet deep had been sunk from this gallery, and branches driven from the bottom of them further into the chalk, in order to obtain greater lines of least resistance, on the level of what would have been the bottom of the proposed tunnel, and agreeing with the position of the rails. Mr. Cubitt had previously told me, *that his rule for estimating the quantity of gunpowder for explosions in chalk was, to use half an ounce for each foot of the cubed line of least resistance.* As he talked of having charges of from 5000 to 6000 or 7000 lbs., I could not but be surprised at this unusual mode of estimating such very large charges by halves of ounces; but, on going to the spot, Mr. Wright explained the mystery by informing me that he had, by Mr. Cubitt's direction, fired four experimental mines in the course of the year, in which, having had no previous personal experience, he adopted the rule laid down by Major General Sir John Burgoyne, formerly of the Royal Engineers, and now President of the Board of Public Works in Ireland, for blasting in hard rock, which, as the line of least resistance in rock seldom exceeds a few feet, it was more convenient to determine in parts of an ounce than in pounds; and he also adopted Sir John Burgoyne's recommendation in his printed paper on blasting, by firing those mines with Bickford's fuses, which is an excellent expedient for blasts in rock, and inferior only to the voltaic battery, but such as I never would have used for mining. For example, Mr. Wright's first experimental mine, fired on the 5th of March, 1842, had a line of least resistance of 25 feet, was loaded with

500 lbs. of powder, had a tamping of 50 feet, and was fired with 100 feet of Bickford's fuses, in two lengths. His three other mines, the largest of which had a charge of 1100 lbs., were each fired by 50 feet of Bickford's fuse. It is difficult to conceive anything more tantalising than these arrangements must have been, for I calculated that 100 feet of Bickford's fuse would burn nearly an hour, and that 50 feet of the same would burn nearly half an hour, before the explosion took place. Nothing can be more teasing than such suspense. Our practice in the Royal Engineers, before we began to use the voltaic battery, was very different, as we never allowed a greater interval between the lighting of the portfire, or fuse, and the ignition of the charge, than from one to two minutes, being just enough to allow time for the officer, or non-commissioned officer, who fired the mine, to retire to a distance sufficient for his personal safety. In all his experimental mines, which were fired singly, and independent of each other, Mr. Wright found that the rule deduced by Sir John Burgoyne with great care and skill from numerous experiments tried by his direction, in order to ascertain the proper charges for blasts in hard rocks, which had heretofore been left to the discretion of the miners, or quarrymen, and which, in practice, seldom exceed a few ounces, were equally appropriate for mining in solid chalk; in which the charges, calculated according to the above rule, produced moderate demolition, without throwing the fragments to a distance—an object always desirable, except in military mines, having not merely demolition, but destruction, in view.

After having had this matter explained, I again inspected the plans and sections of Round-down Cliff, and, considering the length of the gallery and the proposed lines of least resistance, two of which were to be about 56 feet, I was of opinion, from my own repeated experience in conjunct mines, which had not as yet been attempted by the engineers of the South-Eastern railway, that two mines only, with charges calculated to effect moderate demolition, could not possibly throw down the whole of the cliff. I therefore approved of three charges, to be placed at equal distances from each other, but the two extreme charges to be nearer to the ends of the gallery than to the intermediate charge in the centre of it; and as I thought that Sir John Burgoyne's formula must be calculated rather for very moderate, than for moderate, demolition, I was of opinion that the distances between the three charges should be somewhat less than two-lined intervals, which our own experiments on conjunct mines had established as the most proper for moderate demolition. In case of using three charges, Mr. Wright informed me that, whilst the line of least resistance of each of the two extreme mines would be 56 feet, that of an intermediate mine between these two would be about 62 feet, which lines required two charges of about 5,500 lbs., and one of 7,500 lbs., if calculated according to the above formula, without reference to their distances apart. This last point had not yet come under the consideration of the engineers of the South-Eastern Railway, who had only fired single and independent mines, as was before observed.

On giving my opinion first to Mr. Wright, and to Mr. Hodges, his

assistant, and afterwards to Mr. Cubitt, the latter objected, and, as I admitted, on good grounds, to the extreme mines being moved nearer to the ends of the gallery, although this arrangement had been adopted by me with perfect success in all my conjunct mines, because he apprehended that this would cause the fragments from those two mines to be thrown out laterally on each side of the Round-down Cliff, in the direction of the gallery, prolonged so as to obstruct the proposed line of railway, more than if the whole were projected forward towards the sea. At the time when this conference took place, I did not know how many mines were prepared, as I had only gone down one shaft, and into one chamber, to examine, but did not walk through the whole of the gallery, so that the impression upon my mind was that three chambers were then in readiness for the explosion, which was confirmed by my afterwards hearing that three charges corresponding with the lines of resistance then mentioned to me, had actually been adopted; and, therefore, when I wrote to you on the 23d of January, it was natural for me to believe, not only that every arrangement had then been previously fixed by Mr. Cubitt, subject to my approval, but that all the three chambers were actually ready for receiving the gunpowder, and only waited for the voltaic apparatus, which had all to be made, as I recommended, in preference to borrowing.

I have since been informed by Lieutenant Hutchinson, that I was so far mistaken, that only two chambers were prepared at that time, so that the third shaft, with the branch and chamber leading from it, were excavated subsequently to my visit to Dover; and also that the position of one of the first two chambers was altered after the same period. Eventually the three chambers were placed at only 70 feet apart, thus dividing the length of the gallery into four equal parts; but the line of least resistance of the central chamber, on placing them all in the same allinement, proved to be 72 feet, which would have required a charge of nearly 11,700 lbs., according to Sir J. Burgoyne's formula; and yet the original quantity of powder calculated for 62 feet was not altered. These arrangements were thus definitively made, not before, but after, my visit to Dover, at the suggestion of Lieut. Hutchinson, after he (by permission of Sir G. Murray) had been placed in charge of the proposed mines; and were very judicious, because the chambers, being only 70 feet apart, were at much less than two lined intervals, even in reference to the shortest of the lines of least resistance (56 feet) above mentioned; and, in this case, if the central charge had been estimated according to the same formula as the others, violent demolition would have been produced, which was not desirable. In short, the same rule invariably adopted by us in the royal engineer department, in respect to conjunct mines, was here followed—namely, to diminish the regular charges, which are known to be capable of effecting moderate demolition, whenever they are placed at much less than two-lined intervals apart. Though this term is very generally understood, yet, perhaps, it may not be superfluous here to explain, that, in speaking of conjunct mines, the term "two-lined intervals" implies that the central distance between

adjacent charges is twice the line of least resistance of each, the latter being the distance from the charge to the nearest surface of the rock, or mass, that is to be removed by the explosion.

The whole of the arrangements for firing these great charges by the voltaic battery, were made by Lieutenant Hutchinson, assisted by Lance Corporal John Rae and private Thomas Smith, of the Royal Sappers and Miners, and by two naval pensioners, John Leary, a blacksmith, capable also of working in tin or copper, and William Gordon, a rigger; all of whom had been employed under the same officer at Spithead, and who, in their several capacities, understood thoroughly every thing relating to the preparation of charges, and to the mode of firing them by the voltaic battery. Leary, who is an excellent workman, and who distinguished himself, some years ago, whilst under the command of Captain Dickenson of the Royal Navy, by converting ships' tanks into a diving-bell, by means of which that enterprising and intelligent officer recovered the treasure sunk in the Thetis frigate on the coast of Brazil, was employed, on his arrival at Dover, in making voltaic batteries for the proposed explosions, nine in number, each consisting of six cells of Professor Daniell's constant battery, such as had been used by me in all my mining operations; and he also put together the wires for three conducting apparatus, each 1000 feet in length, and, consequently, composed of 3000 feet of copper wire. Each apparatus consisted of a pair of wires attached to a strong rope, and secured and insulated by Pensioner Gordon in the same substantial manner that had been adopted by us at Spithead; for, though there was little necessity for guarding against the action of water, yet the letting it down and dragging it up the high chalk cliffs, exposed this apparatus to a good deal of wear and tear; and it might also have been injured by the hob-nailed shoes of railway laborers, to which it was continually exposed, as I observed particularly on the day it was used, when every person that came near it trod upon it, and which, had it not been thus protected, might have destroyed the connexion, and prevented the explosion, of which I have known instances in the course of our former experiments. As soon as the batteries and conducting apparatus were complete, Lt. Hutchinson tried experiments to ascertain whether he could fire all the three charges simultaneously by one powerful battery, *as had been done by Dr. Hare, of Philadelphia, who first applied voltaic electricity to practical purposes, by using it for blasts in rocks, to obtain stone for building, in* 1831; as minutely described in Silliman's American Journal of Science, vol. xxvi, page 352, and also briefly noticed in the transactions of the British Association for the Advancement of Science, held in Bristol in 1836. From his own experiments, tried with this object, Lieut. Hutchinson drew the same inference that I had done about three years before—namely, that one cannot depend upon more than two charges exploding simultaneously, for though, by a battery of extraordinary power, he succeeded in firing twelve small experimental charges at the distance proposed for the great mines under his direction, yet there was a perceptible interval of time between the reports, which resembled a volley of musketry rather

than the discharge of a single gun. He therefore determined to adopt the plan I had proposed to use in 1839, had it proved advisable to fire four subaqueous charges simultaneously against the Royal George —namely, to have a separate voltaic battery for every charge, and a person at each, with one conducting wire fixed to a pole of the battery, and the other in his hand ready to complete the circuit, according to the time marked by the chief, who was to give the words—*one —two—three*—with an interval of about one second between each, and then the word *fire*, which was to be the signal for completing the circuit; and by this mode I expected that the explosions would all take place simultaneously, on the principle of marking time in music. The powder in each of the three chambers prepared for the several mines at Dover, was contained in bags, placed in a large box, the former expedient having first been adopted in the practice of the Royal Engineers at Chatham; but we never used box and bags also, which I considered superfluous. As these boxes formed what may be called double cubes, Lieutenant Hutchinson very judiciously had a couple of short branches forking out from the lower extremity of each conducting apparatus, into two central points of the oblong charge. Very short and fine pieces of platina were placed, according to custom, near the closed ends of strong tin tubes fixed to the outside, and leading into the centre, of the powder boxes, in which tubes bursting charges of fine powder were introduced, surrounding the platina wires, on the same principle that had been used at Spithead, but without those extreme precautions that had been found necessary to resist the great pressure of water to which our charges there were subject.

In the course of Lieut. Hutchinson's experiments, an unforeseen difficulty occurred, owing to Daniell's batteries, which had been very promising, losing their power after the first frosts set in. This difficulty had never embarrassed us before, because, in our experiments at Chatham, we always took the battery out of a warm room, and it required a longer time to impair its power than our experiments there ever occupied; and at Spithead, where Lieut. Hutchinson first used the battery, it was generally kept in the cabin of one of our lighters; besides which, the work was only carried on during the summer months. He was therefore obliged to have a small wooden shed built for his batteries at Dover, and to keep fires lighted whilst using them, by which means he got rid of the difficulty.

I have since been informed that, in experiments tried at Calcutta, a very energetic battery lost half its power when the temperature fell from 120 to 60 degrees of Fahrenheit. When this difficulty occurred, a prejudice was naturally excited against Daniell's battery, and four very powerful plate batteries were ordered at Dover in consequence, which were made by an intelligent tradesman of that town. The trough of each of these contained 20 cells, according to Dr. Wollaston's construction, with zinc and copper plates, measuring 7 by 10 inches, the latter of which were only let down into the trough when the battery was about to be used; and these plate batteries were combined with the batteries made by Pensioner Leary as before men-

tioned, so that one very powerful battery, consisting of 40 plates of the common system, and of 18 cells of Daniell's constant battery, was to be used for each of the three charges. But here I must remark upon a great inaccuracy in my letter to you of the 23d of January last, in which I stated that the length of conducting wires about to be used at Dover was far greater than had ever been used by me either at Chatham, or at Spithead, instead of which the contrary was the fact; for, on referring back to the journal of our experiments at Chatham, I find that we fired an experimental charge on the 7th of July, 1839, at the distance of 1,950 feet, by 14 cells only of Daniell's constant battery, as recorded in the United Service Magazine for January, 1840; being more than twice the distance at which the great mines at Dover were afterwards fired by batteries of three times that magnitude, and at a temperature which could not have been less than that of our experiment. I said twice the distance, because the conducting apparatus for the charges at Round-down Cliff, originally each 1000 feet long, were afterwards reduced to less than 900, their former length being unnecessarily great. I thought it right to rectify this error, lest a prejudice should be excited against Daniell's constant battery by its supposed inferiority, which led to the employment of plate batteries at Dover, in addition to those of his pattern, which were first made. At the same time, I am now of opinion that the plate battery is the most convenient of the two for firing gunpowder; and the simplest that I have seen is that which is now being used by Mr. R. Davidson, of Aberdeen, in his interesting exhibition of electromagnetic power at the Egyptian Hall, Piccadilly, which I visited lately in company with Dr. Faraday and Mr. Brand. This battery, which contains 20 cells, differs from Dr. Wollaston's in using amalgamated zinc, and in substituting plates of iron instead of copper, all the plates measuring 8 by 11 inches, and the action being produced by diluted sulphuric acid, upon the purity of which, Mr. Davidson says, the efficiency of his battery chiefly depends. On inquiring who first adopted iron plates instead of copper, Mr. Davidson assured me that he had used the former metal himself for about twenty years, but that the merit of this arrangement was disputed by Mr. Sturgeon and Mr. J. Martyn Roberts, with whom he himself had not thought proper to contest it. Dr. Faraday observed, that articles published in any public or scientific journal afforded the only genuine grounds for deciding upon priority of inventions; for the same idea might occur to several persons, and the individual who worked in private must give way to those who published. On this plea, I advise those who ascribe the merit of applying the voltaic battery to the purposes of blasting in earth, or rock, or the peculiar construction and management of the first plate battery, well calculated for this purpose, to any of our own countrymen, to refer to the documents before quoted, and they will find that they are doing an injustice to Dr. Hare, of Philadelphia. But it must not be forgotten, that Mr. William Snow Harris, of Plymouth, was prior even to Dr. Hare, having fired gunpowder by electricity in March, 1823, which he effected, to the astonishment of numerous spectators, by a common electrical machine, from the cabin

of a small vessel at anchor in that port, whilst the charge was placed in another at a considerable distance, and separated from the former by the water, through which his conducting apparatus passed. But the electrical machine, though perfectly efficient, never would have superseded the common modes of firing mines, as the voltaic battery has done, because the former not only requires a much more delicate manipulation than could be expected either from civil or military miners, and would be more easily broken, or deranged; but it also requires artificial heat at all times, even in summer; whereas the voltaic battery can always dispense with this very inconvenient arrangement, even in the depth of winter, except in the case of very long exposure to a low temperature, which can seldom occur.

To return from this digression to the mines near Dover. By the 26th of January, the day appointed for the explosion, all the great charges had been placed in their respective chambers, with the two small bursting charges in the centre of each, whilst the conducting apparatus were led thence, two out of the east, and one out of the west, end of the gallery, to the summit of the cliff, about 300 yards beyond the edge of which they were united with their respective batteries. These were placed alongside of one another in the shed before mentioned, in which powerful charcoal fires were kept burning, one near each battery. The mines had been tamped by filling up the branches and shafts, and ten feet of gallery on each side of the shafts with rammed chalk, but leaving a vacant space of several cubic feet at each chamber, which had not usually been done by me, as my first experiment left me in doubt whether any advantage was obtained from this arrangement. Before the hour appointed for the explosion, the three voltaic batteries, each consisting, as before mentioned, of 40 sets of Wollaston plates and 18 of Daniell's, had been got ready by Lieut. Hutchinson, assisted by Lance Corporal Rae and Private Smith, who had been specially employed under him all last summer in preparing and firing the numerous subaqueous explosions at Spithead. Mr. Wright and Mr. Hodges, who had been present at, and assisted Lieut. Hutchinson in, his preliminary experiments, were now each stationed at one battery in readiness, whilst that officer himself took post at the third, to give the word of command.

The position of the spectators, and the signals for firing, &c., have been so well described by our own reporter, as well as by Sir John Herschell, in the Athenæum, and in other papers of the day, that I shall only remark that considerable anxiety was caused by the unexpectedly long intervals that elapsed between the first and second signals, which led the spectators to apprehend that something had gone wrong, or been forgotten. At last, the second signal was made, and the third signal for firing followed at the appointed interval. At this moment, the lower part of the cliff was seen to swell, or bulge, out, immediately after which the top of it descended gradually, whilst the bottom also was put in motion, and flowed slowly towards, and into, the sea, spreading out, at the same time, to more than its original width; and as it approached and filled up part of the water, a black margin was observed issuing from the extreme outline of this extra-

ordinary stream of white chalk, which was at the time apparently in a fluid state. No smoke was perceived anywhere, unless the dark border, of which no trace remained afterwards, was such. I neither heard a report, nor felt a shock, myself, nor had I anticipated any, from the small quantities of powder used—that is, comparatively small, in reference to the depth at which the charges were buried; but the former was perceptible to many of the spectators of more acute hearing, and the latter was felt also, and described as a slight tremulous motion of the earth, by some of them. It was particularly noticed by those who were seated on the ground near me, at a high point of the cliff to the westward, which commanded a flanking view of Round-down. I would have preferred standing much nearer, and indeed a person at the distance of fifty yards from the edge of Round-down Cliff itself would have been perfectly safe, but it was impossible to have a good view except from a distance. Lieut. Hutchinson and his assistants lost the sight, and, as they felt no shock and heard no explosion, they were not without some apprehension that their mines had failed, until they rushed out of the battery-house, and heard the repeated cheers of the delighted spectators, amongst whom the hardy railway laborers, who are chiefly men of Kent, were not the least vociferous. The cause of the delay between the first and second signals was now explained to me by Lieut. Hutchinson. One of the three batteries, when tested by the voltameter, proved inactive, and, therefore, there was reason to fear that the conducting apparatus of one charge might have been deranged. But, on a closer investigation, it was found that a zinc rod in one of Daniell's batteries had broken by some accident; at which I was less surprised, because I had previously remarked on the very bad quality of the zinc, supplied from London, of which these rods had been made. On discovering this defect, that battery, consisting of six cells, was set aside, and the connexion was made by the remaining twelve, combined, as before stated, with forty sets of Wollaston plates—a power of battery even then far exceeding what was absolutely necessary; but it is best, on great occasions, to employ a superfluity of power, as I myself have always done.

Soon after the sort of volcanic movement caused by the explosion had come to an end, we observed from the top a great number of spectators, who had stood below at a respectful distance from the foot of the cliff, but who now ran and spread themselves over the masses of chalk that had been moved towards the sea, and covered a large space of ground. These persons appeared like pigmies from the high and distant point whence we viewed them; and the moving stream of chalk which flowed towards the sea, when seen from the same point, had previously appeared as if it had been crumbled into white powder, for no part of it seemed larger than the usual size of beach shingle, and the inequalities on its surface were imperceptible; but, on descending the cliff and examining the *debris*, we were surprised to find that they consisted of large irregular fragments of all sizes, some of which must have weighed more than a ton, and which were

heaped up, or packed, on some places, to the height of about thirty feet, but more spread out in others.

Here and there we found fragments of earth and grass that had originally covered the top of the cliff, lying upon those rugged masses of chalk below, which, when seen from above, had appeared like dark brown spots on a white ground. As it was extremely troublesome and fatiguing to walk over the *debris*, for the smaller lumps of chalk rolled under the foot, and the larger ones could not be ascended or passed without an effort, several persons went down to the beach in order to go entirely round them, it being then low water, in which they afforded some merriment by sinking up to their middle, or falling down in crossing some little quagmires of very fine chalk and mud, with small temporary streams flowing through them from the bottom of those great masses, which had prevented the whole of the water of which they took the place from escaping quickly as the tide fell. A flagstaff had been placed at the summit of the cliff before the explosion, which was found prostrate, but uninjured, at some distance from the bottom of it, and was set up again, with a flag of the same color, on the spot, by the railway laborers. I observed a considerable portion of the voltaic conducting apparatus, which had also been thrown down; when afterwards collected and opened, for very little of it was lost, the copper wires were found to be much injured by the kinks occasioned in its fall, but externally it had appeared perfect. That the ruins of the chalk cliff thrown down by this great explosion should have covered fifteen acres of ground, may appear surprising, or even incredible, to many, as, from recollection, it did to me, until I was assured that an accurate survey of it had been made by Mr. Hodges, of which such was the result. The new face of the cliff produced by these great mines was nearly parallel to the original slope, but of a more regular form, being nearly a plane surface, except at the bottom, where a proportion of small chalk rubbish, brought down by the explosion, is piled, at a greater slope, against that which still remains solid.

Gratified, as they could not fail to be, by the splendid results of an operation that probably did not save less than £7,000, the Chairman and Directors of the South-Eastern Railway Company addressed a letter of thanks to the Master-General and Board of Ordnance, on the 16th of February, "for having allowed Lieut. Hutchinson, of the Royal Engineers, assisted by Lance Corporal Rae and Private Smith, to make the arrangements for, and superintend the firing of, the great mines at Dover, on the 26th of January, by which the entire removal of Round-down Cliff was completely effected;" and further observing, "that the important operation referred to having been accomplished by the voltaic battery, with a degree of skill as gratifying to the Directors of the Company, as creditable to the talents of Lieut. Hutchinson and those acting under his directions, they solicited the permission of the Master-General and Board, that Lieut. Hutchinson might be allowed to receive from the Company a piece of plate, which the Directors were desirous of presenting him with, in token of the

high estimation in which his valuable services on the memorable occasion referred to, were held by them."

This proposition having been acceded to by the Master-General and Board, "as a special case," for it is contrary to etiquette that the services rendered by an officer of the army should be noticed by any mark of approbation, except by his own superiors, if performed as a part of his military duty, or by their permission, if otherwise,—as soon as this was communicated to them through Mr. Byham, the sum of fifty guineas was expended by the Chairman and Directors of the South-Eastern Railway Company in this testimonial of their gratitude to Lieut. Hutchinson.

Finding the immense benefit of this great explosion, Mr. Wright and Mr. Hodges, by the approbation of their chief, have since fired several other mines with equal success, in the same range of chalk cliffs, by the voltaic battery, of which they acquired a thorough knowledge, as well as of the general principle by which conjunct mines ought to be regulated, whilst under Lieut. Hutchinson—viz., two mines of 750 lbs. each on the 10th, and two of 900 lbs. each on the 14th, of February; after which, on the 2d of March, they fired eight conjunct mines, all in the same line, in which they expended 6,440 lbs. of gunpowder, along a range of cliff of such very irregular outline that they varied their charges from 200 lbs. to 2000 lbs. Mr. Hodges has contrived a simple and ingenious apparatus for completing the circuit of several voltaic batteries at the same moment, by one operator; and they propose to fire more than 12,000 lbs. of powder, distributed amongst fifteen or sixteen conjunct mines, in order to remove another portion of the same cliff, on the 18th instant. In short, these gentlemen, who had no knowledge of this art a year ago, have profited so well by their opportunities, that I consider them capable of planning and executing any mining operation, however extensive, with skill and success.

In respect to conjunct mines calculated for a scale of moderate demolition, I shall here remark, that it is not absolutely necessary that they should all explode at the same moment of time, (which is difficult even by the voltaic battery, but, by all the former methods of firing gunpowder, absolutely impossible, though the contrary has been asserted by writers copying from the old French authors on military mining.) For example, in the course of our experiments at Chatham, before we knew the use of the voltaic battery, we first demolished a brick wall, about four feet thick, by blasts fired successively by a very small powder hose leading along the back of the wall, and connected with each charge, one end only of which hose was ignited. In like manner, Lieutenant (now Captain) James, of the Royal Engineers, acting under my direction, demolished 466 feet in length of the brick revetments, or retaining walls, of an entire front of the old fort of Sheerness, with the exception of one of its flanks, but including its ravelin, with such complete success, on the 14th of July, 1827, that the brickwork was, as it were, just turned over nearly on the same spot, but crumbling into pieces as it fell, without any of the fragments being thrown to a distance, although no two of the explosions were

simultaneous. In this operation, 15 charges, generally of 84 lbs., but some of 90 lbs., in barrels, were used against the demibastion and curtains, whilst 23 charges of 25 lbs. each, placed in bags, were used against the ravelin. The former were grouped by twos or threes as conjunct mines; the latter were all fired one after another, at intervals of several seconds of time, by igniting one end of a longitudinal hose laid along the top of the wall, and communicating with separate vertical hoses leading down into the several charges. But though simultaneous explosion is thus evidently unnecessary in conjunct mines, having their charges calculated to effect moderate demolition —yet, in the case of violent demolition, absolute precision is indispensable, otherwise the explosion of the first charge may derange the others, and either diminish the general effect of the whole, or even cause some of them to fail altogether, of which I have known instances.

The great explosion which took place at Dover on the 26th of January last, is certainly the triumph of the art of mining in this country, but the British military engineers, by whom undoubtedly it has been brought to perfection, were not so fortunate in their first attempts as the able civil engineers of the South-Eastern Railway. Owing to the inefficient state of the Royal Engineer Department at the commencement of the present century, which was not improved until towards the close of the Peninsular war, the officers had no opportunities of acquiring practical knowledge of this important art. Hence in Sir John Moore's retreat all the mines of demolition made for the destruction of the bridges in our rear failed, excepting one, in which Lieutenant Davy, a most promising young officer, blew himself up along with the bridge that he destroyed; and I myself was one of the unsuccessful operators in that campaign, in an undertaking in which no non-commissioned officer, and very few privates of the corps would now fail. At the siege of Badajoz, Captain Stanway, a most gallant and intelligent engineer officer, succeeded in placing a charge to blow up a dam that retained an inundation, which was a great impediment to the besiegers, but the explosion failed, from the mode of securing charges against the pressure of water not being then understood; and in the attack of Burgos, the work of the British miners was obliged to be suspended from time to time, for want of air, because the simple method of ventilating military mines was not known. Towards the close of the Peninsular war, however, the distinguished engineer officers employed in it had acquired more experience, and none of their mines of demolition against bridges, &c., failed as at first.

In India, where the same defects prevailed, perhaps to a still greater extent, the first mining operations after the commencement of the present century were not merely unsuccessful, but calamitous, for at the siege of Cumoona, an insignificant little mud fort, in 1807, the company's engineer officer, whilst preparing mines to throw down the enemy's counterscarp, was himself blown up in his own gallery, by the native miners opposed to him, and our troops were afterwards repulsed with great slaughter in an attempt to storm the place, which,

however, was afterwards evacuated in the night. About two years before, in the first siege of Bhurtpore by Lord Lake, the repeated assaults ordered by that gallant general had been repulsed, chiefly by explosions of gunpowder prepared in the ditch. More recently, the East India Company's Engineers have everywhere distinguished themselves by their superior skill in mining, especially at the second siege of Bhurtpore, in 1826, though the native miners there had lost nothing of their former energy, for they actually penetrated into one of our galleries, where a combat took place, in which a captain of the Engineers was wounded. Their efforts were, however, unavailing, for the Company's Engineers opened the way for our storming parties into that supposed invincible fortress by two great mines, one of which not only effected a broad and practicable breach, like the other, but leveled in the dust a large circular bastion, on which 300 or 400 of the enemy's bravest troops were posted, who were all destroyed by the explosion. I need scarcely mention that the East India Company's Engineers performed the same important service in Lord Keane's attack of Ghunzee in 1829. Failures on either of those occasions, such as had previously occurred after the beginning of the present century, would have shaken our Indian empire to its centre. In Europe and America, where the Royal Engineers only are employed, mining has not been required in any of our military operations subsequent to the close of the Peninsular war; so that they have not had the same opportunities of establishing their character for skill in mining, which their friends and contemporaries of the same branch of service in India have enjoyed.

If my first letter, which appeared in your columns of the 24th January last, had not been full of inaccuracies, I would not have troubled you either with the above account of the great mines at Dover, or with the observations on the progress of the art of mining in the British service, into which I was led in the course of my narrative, for the length of which I beg to apologize, and am, Sir,

Your most obedient servant,

C. W. Pasley, *Major-General.*

Civ. Eng. & Arch. Journ.

Architecture.

Tesselated Pavements—Ancient and Modern.

[A work has been recently published, at a great expense, under the direction of Mr. Blashfield, who is connected with the old established firm of Messrs. Wyatt, Parker & Co., the cement manufacturers, for the purpose of exhibiting to the profession what truly beautiful patterns may be adopted in tesselated and mosaic pavements, by the aid of the small porcelain squares recently introduced by Mr. Blashfield for that purpose. The work consists of ten elaborate designs by Mr. Owen Jones, the author of the "Alhambra," splendidly printed in colours. These designs cannot fail in directing the public taste to

this admirable description of ornament, for the floors of halls, saloons, conservatories, baths, &c.—we may also add the aisles of churches; for to our taste, it is far preferable to the dingy encaustic tiles. The following essay on the materials and structure of tesselated pavements is by Mr. F. O. Ward, who has devoted considerable research in collecting the information.]

The object of the following notice is to call public attention to a new material for tesselated pavements, and to an improved method of constructing the same, by the adoption of which this ancient and esteemed mode of decoration may be re-introduced, at a moderate cost, for the embellishment of our modern buildings. The improvements in question will, it is confidently believed, enable the modern architect to execute mosaic floorings, equal in point of extent and elaborateness to the most celebrated of the remains that have descended to us from antiquity, and very far superior to these in brilliancy and variety of colouring, in the accurate co-adaptation of the pieces, and in the uniform durability of the surface.

In order to arrive at a just conclusion on this subject, it will be necessary in the first place to bestow some attention on the materials and structure of the old Roman tesselated pavements, as described by Vitruvius, and still to be traced in the remains existing in various parts of the country, and in the specimens preserved at the British Museum.

The materials of the best and costliest pavements at Rome (such, for example, as those still remaining in the baths of Caracalla,) are colored marbles of various kinds, differing considerably from each other in hardness and durability. The inferior pavements, found scattered through Britain, France, and other parts of Europe, and along the Northern coast of Africa, are usually made of such coloured stones as the neighborhood happened to supply, with the exception only of the red tesseræ, which are almost invariably of burnt clay. Thus, in the celebrated Roman pavement which was discovered in 1793, at Woodchester, in Gloucestershire, the gray tesseræ are of blue lyas, found in the vale of Gloucester,—the ash-coloured tesseræ of a similar kind of stone, often found in the same masses with the former, —the dark brown of a gritty stone, met with near Bristol, and in the forest of Dean,—the light brown of a hard calcareous stone, occurring at Lypiat (two miles from the site of the pavement)—and the red tesseræ, as usual, of fine brick. These materials differ from each other in point of hardness even more than the colored marbles of the costlier pavements at Rome; and it is evident that a surface composed of such heterogeneous materials must wear unequally at different parts, and ultimately fall into hollows wherever colors produced by the softer kinds of stone are employed.

If this remark should be met by a reference to remains of ancient pavements, discovered in this country after a lapse of sixteen centuries from their first construction, and still retaining a level, unworn surface, it is obvious to reply, that the mere length of their duration gives no force to the objection, seeing that, during by far the greater portion

of the time, these pavements have lain buried; and, further, that even when in use they formed floors to the baths and best chambers of the residences of Roman provincial governors, and were, therefore, doubtless, subject to very inconsiderable traffic. The entrance hall of a modern club-house would afford a much more trying test of durability; and it will hardly be disputed that a pavement composed of heterogeneous materials would in such a position be liable to wear unequally.

The next point to be observed with reference to the Roman tesseræ, is the want of uniformity in their size and shape, and the consequent irregularity of their junctures, especially in the more minute portions of the design. Whoever will take the trouble to examine the choicest specimens of old pavements at the British Museum, (as, for example, one presented by Mr. Lysons, which formed part of the Woodchester pavement referred to above,) will perceive that the tesseræ, instead of coming into contact by smoothly ground and equal sides, are in many places separated by broad and uneven lines of cement. In some parts the intervals are of such width that the cement, which in a good pavement would be scarcely seen, forms at least a fourth of the visible surface. It is scarcely necessary to point out the effect which this net-work of brown cement lines, running through the whole design, and mixing a muddy hue with every tint, must have in diminishing the purity of the colors, and in deadening the sharpness and brilliancy of their contrast. It is much as if a picture, when finished, should be crossed and re-crossed all over with lines of brown paint.

Proceeding from these remarks on the materials of the Roman pavements to consider the mode of their construction, we shall find that, while the effect produced was imperfect, the means employed for its production were costly and inadequate to the end proposed.

Vitruvius, in the first chapter of his seventh book of architecture, after describing the manner in which the foundation of the pavement should be formed, goes on to say, that on the topmost layer of cement the tesseræ are to be laid—care being taken to keep the surface flat and true with the level; that, in the next place, all unevennesses and projections are to be worked down by rubbing and polishing; and that, lastly, a layer of cement is to be spread over the whole and scraped off again (in order, it would seem, to fill up any cavities in the cement between the tesseræ, and to render the surface as smooth as possible all over.)*

We need not dwell at length on the time and trouble that it must have taken to set each tesseræ separately in the cement, and to try the surface with the level after every few pieces were laid. With respect to the subsequent operation of grinding down and polishing the surface of the work, it must have been in most cases, (and particularly where stones of a hard and gritty nature were employed) the most tedious and laborious part of the process. We shall pre-

* This is the general sense of the passage, according to the best commentators. The phraseology in the original is here very obscure, and has probably suffered from the carelessness of early transcribers.

sently see that all these difficulties are obviated by the employment of the newly invented material and mode of construction, which we will next proceed to describe—taking, however, in the first place, a rapid survey of the various experiments which preceded this invention, and of the successive improvements by which it has been gradually brought to perfection.

About forty years ago, a patent was obtained by Mr. C. Wyatt, for a mode of imitating tesselated pavements, by inlaying stone with colored cements. Floors thus constructed, however, were found liable to become uneven in use, in consequence of the unequal hardness of the materials; which defect prevented their general adoption. Terra cotta inlaid with colored cements has also been tried, and found liable to the same objection.

During the last ten years, cements colored with metallic oxides, have been used by Mr. Blashfield, to produce imitations of the ancient tesselated pavements; and, for work protected from the weather, the material appears to have answered tolerably well; but for out-door work, required to stand frost, it has been found necessary to employ Roman cement, the dark brown of which gives a dingy hue to all colors mixed with it. This, with some other practical difficulties, has interfered with the success of the plan.

Bitumen colored with metallic oxydes has also been tried by Mr. Blashfield, as a material for ornamental floorings. The groundwork of the pattern was first cast in any given color, and the interstices were afterwards filled up with bitumen of various other shades. But this method was even less successful than the former; the contraction and expansion of the bitumen soon rendered the surface uneven; the dust, trodden in, obscured the pattern; and the plan, besides being ineffectual, was expensive.

Three years ago, Mr. Blashfield succeeded in constructing an extensive and elaborate inlaid pavement, on the plan of the Venetian Pisé floors. It was made after designs furnished by H. S. Hope, Esq., at whose country-seat, Deepdine, in Surrey, it was laid down. It is still in good preservation.*

In the same year (1839) Mr. Singer, of Vauxhall, obtained a patent for a mode of forming tesseræ, by cutting, out of thin layers of clay, pieces of the required form, which are afterwards dried and baked in the usual way. His patent also included an improved method of uniting the tesseræ with cement, so as to form slabs of convenient size for paving. He has executed in this manner some very admirable mosaics, and his invention must be regarded as one of the most important steps towards the revival of the art in this country.

We now come to the discovery which led to the invention of the tesseræ particularly referred to throughout this treatise.

In 1840, Mr. Prosser, of Birmingham, discovered that if the material of porcelain (a mixture of flint and fine clay) be reduced to a dry powder, and in that state be subjected to strong pressure between steel dies, the powder is compressed into about a fourth of its bulk;

* A floor of a very similar kind was laid down at Mr Hope's mansion, in Duchess street, about sixty years since, and it is said to be still in excellent condition.

it then undergoes a process of semi-vitrification, and is converted into a compact solid substance, of extraordinary hardness and density; much less porous, and much harder than the common porcelain, uncompressed.

This curious, and as it has since proved, very important discovery, was first applied to the manufacture of buttons, to supersede those of mother-of-pearl, bone, &c. The buttons thus stamped out of porcelain powder are capable of resisting any pressure to which they are subject in use, and are more durable, as well as cheaper, than buttons of the materials ordinarily used.

The applicability of this ingenious process to the manufacture of tesseræ for pavements, soon afterwards occurred to Mr. Blashfield; who made arrangements with Messrs. Minton & Co., (the manufacturers appointed to work Mr. Prosser's patent,) for a supply of small cubes made according to the new process; these he submitted to various trials and experiments, and having found them in every respect suitable for the purpose, he has recently, in conjunction with Messrs. Wyatt, Parker & Co., carried out the invention on an extensive scale. Tesseræ of various colors and forms—red, blue, yellow, white, black, brown; quadrilateral, triangular, rhomboidal, hexagonal, &c.—have been manufactured on this principle in large numbers; pavements of considerable extent have already been constructed with them; and they have been found to possess the following advantages:—

First, being formed in similar steel dies, they are of uniform size and shape, so that they can be fitted together accurately in the laying down of the most complicated designs. Secondly, being all composed of the same material, variously colored, they are all of precisely equal hardness, so that pavements made with them are not liable to fall into hollows in use. Lastly, owing to the effect of the intense pressure under which they are made, they are quite impervious to moisture, of flinty texture throughout, and, in a word, to all intents and purposes absolutely imperishable.

In these several respects, their superiority to the Roman tesseræ, (which, as we have seen, were shaped imperfectly by hand, and differed from each other in hardness,) must be manifest to the reader. Nor less conspicuous is the superiority of the modern process of uniting the tesseræ to form pavements.

For this purpose (instead of spreading the cement on the surface to be paved, and laboriously setting each single tesseræ in it, according to the directions of Vitruvius,) the pavement is first put together, face downward, on a smooth surface, so that the tesseræ find their level without any trouble to the workman; and as soon as a sufficient portion of the design is finished, it is backed with fine Roman cement, which is worked in to fill the crevices between the tesseræ; the pavement is thus formed into smooth flat slabs of convenient size (according to Mr. Singer's method) and these are laid down on a foundation properly prepared in the usual way.

One peculiar feature of this process is, that private persons, if so inclined, may set out their own pavements in the colored tesseræ,

leaving it for a workman afterwards to cement and lay down the slabs. Fine mosaic work for the tops of tables, for illuminated monuments, &c., may be made in the same manner with a superior kind of tesseræ, glazed on the surface, and richly ornamented in gold and colors.

Pavements thus constructed are singularly beautiful. The outline of the design strikes clearly and sharply upon the eye, and the brilliant colors of the tesseræ are reflected from the level surface, uninterrupted by those broad, uneven lines of cement, which in the Roman pavements detract so much from the general effect. The truth of every line and angle in the figure, and the just proportion of all its parts, however complicated and various, impress the mind with an agreeable sense of order and precision. Such, indeed, is the exactness and facility of the workmanship in these pavements, that the oblique and intricate intersections of the Mauresque designs are as readily executed as the simple rectangular patterns of the Pompeiian style. Even the scrolls and twisted guilloches, the quaint emblematical device, and grotesque representations of horses, warriors, &c., found in the most elaborate of the Roman pavements, may be accurately imitated with the new stamped tesseræ.

The Roman designs, however, have little to recommend them to the modern artist, beyond their historical interest. Even the earliest of them, which are the best, were produced subsequently to the Roman invasion of Greece, when art was everywhere declining; and they abound with indications of the extravagant and licentious taste which grew up amidst the general corruption of Roman manners, occasioned by the rapid influx of foreign wealth, and foreign habits of luxurious excess.

When designs after the antique are required, the elements of them should rather be sought in the beautiful decorations of the Etruscan vases, and in the admirable remains of Greek art in general, during its best period—*i. e.* from about 400 to 200 B. C. or during the time of Phidias, Praxiteles, and their immediate successors. (Such are the models which have guided the composition of the magnificent tesselated pavement designed by Mr. Barry, and executed under his direction by Mr. Singer, for the hall of the New Reform Club; a pavement so beautiful and so generally admired, that it can hardly fail to give an impulse to the re-introduction of mosaic decoration hitherto so sparingly employed by modern architects.)

For Mauresque designs, the mosaic dados of the Alhambra may be advantageously consulted. They are executed in glazed earthen tiles, variously colored, shaped with considerable exactness, and joined with cement. They present many examples of ingenious arrangement and well contrasted coloring.

But, whichever of these various styles the architect may adopt, he will find that, for the realization of his conceptions, there is no material which presents so many advantages as the compressed porcelain tesseræ—whether on account of their uniform size and shape—the purity and brilliancy of their colors—or their extreme hardness, and unalterable durability.

Civ. Eng. & Arch. Jour.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

On Public Cemeteries. *By* JOHN C. TRAUTWINE, *Collaborator on Architecture.*

The following extracts from an article on Public Cemeteries, will doubtless prove highly acceptable to many readers of the Journal. It is selected from an able treatise on the subject, written by Mr. J. C. Loudon, of London, Conductor of the Gardener's Magazine, the Architectural Magazine, and other periodicals and architectural works, which have acquired for him, in the United States, a well merited reputation as a gentleman of refined taste, and close observation.

We have begun in this country to give the Public Cemetery system a fair trial; and the entire success, and universal satisfaction which have hitherto attended the experiment, furnish every assurance that the system will eventually become general; while the consequent desire to obtain the most authentic information on the subject, must ensure a cordial reception to a paper like the following, emanating as it does from so high a source.

The extension of the system will be attended by many important advantages to the public. Not only will it remove from our cities a source of disease, much more noxious than is generally supposed, but it will exert a powerful influence in exciting and diffusing a taste for that delightful branch of ornamental agriculture, landscape gardening, which has hitherto remained comparatively unpracticed, and, indeed, almost unknown in this country. Landscape gardening, when conducted on scientific principles, constitutes one of the most agreeable and essential accessories to the beauty of rural architecture; and when our cemetery companies possess the advantage of spots so eminently calculated for its display, as Laurel Hill, Monument Cemetery, Mount Auburn, and others, a free application of its principles cannot fail to arrest the attention of every observer endowed with a spark of natural taste, and gradually lead to its more general subserviency to architectural purposes. Hence I do not hesitate to recommend the introduction of this paper under the head of "Architecture," in the Journal of the Institute.

The erection of monuments designed on architectural principles, contrasting as they necessarily will, with those common-place designs which impart to our grave-yards an air of funereal stereotyping, must likewise aid in improving the architectural taste of the visitors. Even the less observant will perceive that the most costly and profusely decorated monuments are not necessarily the most beautiful; many will thus be led to inquire what contributes the elements of beauty in architectural design? and this inquiry will by degrees extend itself to our public buildings, until the mass of our citizens shall at length derive from the contemplation of a structure erected in conformity with strict architectural principles, some degree of that refined gratification, which is now confined to a very few.

The Principles of Landscape-Gardening and of Landscape-Architecture applied to the laying out of Public Cemeteries and the Improvement of Churchyards; including Observations on the Working and General Management of Cemeteries and Burial-Grounds. By J. C. Loudon, F. L. S., H. S., &c.

The circumstance of being employed by the Directors of a Cemetery Company at Cambridge, to form a plan for their guidance in arranging the ground, and in working and managing the cemetery afterwards, led us to study the principles on which all the arrangements connected with cemeteries are, or ought to be, founded, and the following pages contain the general results of our inquiries.

I. THE USES OF CEMETERIES.

As, to know the best mode of applying the principles of design to any particular object, it is necessary to know the purposes for which that object is intended, we shall commence by considering the *uses* for which cemeteries or burial-grounds are required.

The *main object* of a burial-ground is, the disposal of the remains of the dead in such manner as that their decomposition, and return to the earth from which they sprung, shall not prove injurious to the living; either by affecting their health, or shocking their feelings, opinions, or prejudices.

A *secondary object* is, or ought to be, the improvement of the moral sentiments and general taste of all classes, and more especially of the great masses of society.

With respect to the first and most important object, the decomposition of the dead, without the risk of injury to the living, there is, as we think, but one mode in which this can be effected, to which there can be no objection on the part of the living; and that is, interment in a wooden coffin in the free soil, in a grave 5 or 6 feet deep, rendered secure from being violated, in which no body has been deposited before, or is contemplated to be deposited thereafter.

Various circumstances, however, into which it is needless to inquire, have given rise to burying several bodies in the same grave in the free soil, and to modes of sepulture by which the decomposition of the body, or at least its union with the earth, is prevented; such as the use of leaden or iron coffins, and depositing them in vaults, catacombs, and other structures, in which they can never, humanly speaking, except in the case of some great change or convulsion, be mingled with the soil, or, in the beautiful language of Scripture, be returned to the dust from which they sprung. Though we are of opinion that the modes of burial which prevent the body from mixing with the soil, which, for the sake of distinction, we shall call the sepulchral modes, cannot, on account of the danger to the living, be continued much longer in a highly civilized country, yet, in considering the conditions requisite for a complete cemetery suited to the present time, the various modes of sepulchral burial at present in use must be kept in view. The expense of the sepulchral mode, however,

confines it to the comparatively wealthy; and hence by far the greater part of burial-grounds always was, and is, necessarily, devoted to interments in the free soil. In some churchyards where there is abundance of room, only one coffin is deposited in a grave; but in most cases, and particularly in the burial-grounds of large towns, the graves are dug very deep, and several coffins, sometimes as many as a dozen, or even more, according to the depth of the grave, are deposited one over another, till they reach within 5 or 6 feet of the surface. Interments in this manner are of two kinds. The first are made in family graves, in which the different members of the same family are deposited in succession, in the order of their decease; and to such graves there is always a grave-stone or some kind of monument. The second are what are called common graves, to which there is no monument, and in which the bodies of the poor and of paupers are deposited, in the order in which they are brought to the cemetery; probably two or three in one day, or possibly as many in one day as will fill the grave. Unless this mode were adopted in the public cemeteries, they would, from their present limited extent, very soon be filled up. Such graves, whether public or private, in the newly formed cemeteries, when once filled with coffins to within 6 ft. of the surface, are understood never to be reopened; but, in the old burial-grounds, they are in many cases opened after being closed only four or five years, and sometimes much sooner.

When the parties burying cannot afford to purchase a private or family grave, the practice is, in some burial-grounds, to bury singly in graves of the ordinary depth of 6 or 7 feet, and these graves are reopened for a similar purpose in six or seven years; but, as this is attended with the disinterment of the bones, it is a very objectionable mode. In a burial-ground properly arranged and managed, a coffin, after it is once interred, should never again be exposed to view, nor a human bone be disturbed. At present this is only the case in the cemeteries of the Jews, where there is a separate grave for every coffin, and where the graves are never reopened. It is also the case in the cemeteries of the Quakers; though not, we believe, from religious principle, as in the case of the Jews, but rather from that general regard to decency and propriety which is a characteristic of that sect of Christians, and perhaps, as in the case of the Moravians, in consequence of their comparatively limited number.

As *data* to proceed upon with reference to interments in the free soil, it is necessary to state that the muscular part of the body either decays rapidly, or dries up rapidly, according to the circumstances in which it is placed; but that the bones do not decay, even under circumstances the most favorable for that purpose, for centuries.

The face of a dead body deposited in the free soil is generally destroyed in three or four months, but the thorax and abdomen undergo very little change, except in colour, till the fourth month. The last part of the muscular fibre which decays is the upper part of the thigh, which in some subjects resists putrefaction for four or five years. In general, a body is considered unfit for dissection after it has been interred eight or nine weeks. In a very dry and warm soil, especially

where the body is emaciated, the juices are rapidly absorbed; and, no moisture coming near it, the solids contract and harden, and a species of mummy is produced. This may be observed in the vaults of various churches in Britain where the soil and situation are remarkably dry; and it has given rise to those appalling scenes which may be witnessed in the vaults of Bremen, Vienna, Rome, Naples, Palermo, Malta, and other places. (See *Necropolis Glasguensis*, p. 48. to 55.; and Stephens' *Incidents of Travel*, as quoted in the *Saturday Magazine*, vol. xx. p. 141.)

Bones are chiefly composed of phosphate of lime deposited in gelatine, an animal tissue; and, unless acted on by powerful acids, they will endure, either in the soil or in the atmosphere, for many centuries. They are even found in the fossil state, and after ages of exposure often contain more or less of the original animal tissue, particularly if they have been embedded in clayey soil. In the antehominal part of the creation, there are bones daily discovered which have existed 6000 years at least. Dr. Charles Loudon informs us that he has seen numerous human bones in certain caves near to Naples, which are supposed to be those of the Grecian colonists who settled there before the Christian era, or perhaps those of an older race who inhabited Magna Græcia. Dr. Loudon has seen several skeletons dug out of the ruins of Pompeii, the bones of which were as dry and entire as the bones of skeletons which we see in dissecting-rooms, though they must have lain there nearly 1800 years under the lava, which, around them, seemed to be a dry greyish kind of earth. Even while writing this, we read in the newspapers (*Morn. Chron.*, Jan. 10.) of the workmen, while digging a deep sewer in Lad Lane in the city, having cut into what is supposed to have been a cemetery of the Romans, and dug up a number of human bones.

With respect to *prejudices*, there is, as every one knows, a decided prejudice in favour of being buried in dry soil, and against the placing of decomposing substances, such as quicklime, in coffins; and it is one of our principles to respect existing prejudices as well as vested rights. With regard to the use of quicklime; independently of the existing prejudices against its introduction into coffins, it is found to cause the solution of the softer parts of the body, which, unless the coffin is watertight, and this is rarely the case with the coffins either of the poor or of the middling class, oozes out to such an extent that the undertaker's men can scarcely carry the coffin, on account of the flow of matter and the odour.

The health of the living is chiefly affected by a certain description of gas, respecting which it is necessary to enter into some detail. The decomposition of the muscular part of the human body takes place with different degrees of rapidity in different soils, and at different depths in the same soil. It is most rapid in sandy soils somewhat moist, within 3 or 4 feet of the surface, and in a warm climate; it is next in rapidity in chalky soils; much slower in clayey soils; and slowest of all in peaty soil, saturated with astringent moisture. In general, dry soil, and a moderate distance of 5 or 6 feet below the

surface, are favorable both to decomposition and human prejudices. In such soil, in the climate of London, the muscular part of the human body will have become a black mould in between six and seven years; but, practically speaking, the bones may be considered as indestructible. In the progress of decay, the first change which takes place immediately after death, is the escape of a deleterious gas from the mouth and nostrils, but generally in so small a quantity as not to be perceptible for three or four days. In some cases, it is perceptible in a much shorter period; and in all, a gas accumulates within the body, which escapes sooner or later according to the progress of the putrescent process. If the body is buried in the free soil, in a wooden coffin, to the depth of 5 or 6 feet, the gas escapes into the soil, and is, in part at least, absorbed by it, and consequently does not contaminate the air above the surface; but, if a leaden coffin is used, and the body is deposited in a vault, catacomb, or brick grave, the gas escapes within the coffin, and either remains there till the coffin decays, or escapes through crevices in the lead, and through small holes bored on purpose by the undertaker in the outer wooden coffin and leaden inner coffin, and concealed by the name-plate. *(Report on the Health of Towns, Walker, &c.)* By the last mode the gas begins to escape before the corpse is taken from the house; and its effect is often felt there, as well as when the service is being read over it in the chapel, and even after it is deposited in a vault, the catacombs of which, though apparently hermetically sealed, are seldom air-tight. Sometimes the body, especially of a corpulent person, swells so much before it is removed from the house, that it is ready to burst both the inner and the outer coffin; and in that case it requires to be tapped, and the gas burnt as it escapes, or the operation performed close to an open window. Even in some of the public catacombs of the new London cemeteries explosions have been know to take place, and the undertaker obliged to be sent for in order to resolder the coffin; which shows the disgusting nature of this mode of interment, and its danger to the living. To inhale this gas, undiluted with atmospheric air, is instant death; and, even when much diluted, it is productive of disease which commonly ends in death, of which there is abundant evidence in Walker's *Grave-Yards*, and the Parliamentary Report quoted. The gas abounds to a fearful extent in the soil of all crowded burial-grounds, and has been proved to be more or less present in the soil thrown out of graves where bodies have been interred before. Even in the new London cemeteries, when interments are made in family graves, or common graves, which have been filled in with earth, such is the smell when the grave-diggers arrive within 2 or 3 feet of the last deposited coffin, they are obliged to be plied constantly with rum to induce them to proceed. This is more particularly the case when graves are dug in strong clay, because the gas cannot escape laterally as in a gravelly or sandy soil, but rises perpendicularly through the soil which has been moved. The remedy for this evil is, never to allow a family grave, or a common grave, in which an interment has been made, to be excavated deeper than within 6 feet of the last deposited coffin; and, to make sure of this, there ought to

be a protecting stone, or slate, to be hereafter described, deposited when the grave is being filled, at the height of 6 feet above the last coffin, under a severe penalty. It is only by some regulation of this kind, that burying several coffins in deep graves can be conducted without injuring the health of grave-diggers; and without the gas, which escapes from the earth brought up, endangering the health of those who may be occasional spectators.

In the years 1782 and 1783, when the disinterment of the burying-grounds of Les Innocents in Paris, took place under the direction of some eminent French chemists, these philosophers endeavoured to analyze this gas, but were unable to procure it. Fourcroy, speaking in their name, says:—"In vain we endeavoured to induce the grave-diggers to procure any of this elastic fluid. They uniformly refused, declaring that it was only by an unlucky accident they interfered with dead bodies in that dangerous state. The horrible odour, and the poisonous activity of this fluid announce to us that if it is mingled, as there is no reason to doubt, with hydrogenous and azotic gas holding sulphur and phosphorus in solution, ordinary and known products of putrefaction, it may contain also another deleterious vapour, whose nature has hitherto escaped philosophical research, while its terrible action upon life is too strikingly evinced. These Paris grave-diggers know," Fourcroy adds, "that the greatest danger to them arises from the disengagement of this vapour from the abdomen of carcasses in a state of incipient putrefaction." (See *Annales de Chimie*, vol. v. p. 154, as quoted in Walker's *Grave-Yards*, p. 86; and Ure's *Dictionary of Chemistry*, art. Adipocere.)

While this inflation from gas is going forward, the aqueous part of decomposition, a "fetid sanies," exudes from the body, and sometimes, when interment is delayed too long, to such an extent as to drop from the coffin before it is taken out of the house. This exudation, as already observed, is greatly accelerated and increased by putting quicklime into the coffin. In the free soil this feted sanies is diffused by the rain into the subsoil, and carried along in the water of the subsoil to its natural outlet, or to the wells which may be dug into it; and thus, while the gas of decomposition poisons both the earth and the air, the fluid matter contaminates the water.*

* Speaking of the infectious agency in the houses in the neighborhood of that part of London called Fleet Ditch, Dr. Lynch observes:—"The great primary cause is, that the privies are in general under the staircase of the wretched hovels of the poor, and the sulphuretted hydrogen, and the carbonated hydrogen, and the noxious gases there generated, are the same gases as are generated from the dead bodies in a state of decomposition; for the evacuations from the body are decomposed animal and vegetable matter, and a dead body is the same, it is decomposition of the dead body, or a general state of disorganisation, and that produces exactly the same kind of gases. There have been instances mentioned, where people have fallen down dead from a rush of those gases in a concentrated form." (*Report on Health of Towns*, &c., p. 161.)

If the public were fully aware of the dangerous nature of the gases which proceed from the decomposition of dead bodies in crowded churchyards, and in vaults and catacombs, and of the poisonous nature of the water of decomposition:

1. They would not live in houses bordering on churchyards, which, though already full, are still used as burial-grounds.

2. They would not drink the water of wells dug in the vicinity of burial-grounds, whether

With regard to the *destruction of human bones*, we assume that to be impracticable, otherwise than by means which are altogether out of the question. The most favorable soil for their decomposition is a coarse gravel, subject to be alternately moist and dry; but, though such a soil, so circumstanced in regard to water, might be found naturally, or might be composed by art, yet these cases may be considered as equally impracticable. Instead, therefore, of endeavouring to destroy the human skeleton, let us limit our endeavours to preventing it from being desecrated by disinterment and exposure. This may be effected in various ways; but by far the most simple, effectual, and economical, as it appears to us, would be to place over the coffin, after it was deposited in the grave, a stone or slate of the same dimensions as the coffin, or even as many flat 12-inch tiles, say six, as would extend from head to foot. As the coffin and the muscular part of its contents decayed and sunk down, the stone, slate, or tiles, would follow it and press close on the bones. In consequence of this arrangement, when the ground was at any future period opened to the depth of the stone, slate, or tile, guard, it would be known that a skeleton was beneath, and the operator would cease to go farther; or, at all events, it should be rendered illegal for him to do so. If a name and date were graven in the stone, being protected from atmospheric changes, it would remain uninjured for ages, and, like the foot-marks which geologists have found in the red sandstone, might, in some far distant age, become part of the geological history of our globe. We prefer stone or tile guards, to guards of metal, because iron would soon rust, and cease to be a guard, and lead or any equally durable metal would offer a temptation to stealing. A layer two or three inches thick of stucco, Roman cement, or a plate of asphalte or oropholithe, might be used as a substitute; but stone, slate, and tiles are decidedly preferable. The slate might even be introduced within the coffin, without rendering it heavier to carry than if a lead coffin were used. Burying in a coffin made entirely of stone or slate we do not consider so likely to prevent desecration as a stone or slate guard; because there is a temptation to dig up the lower part of the stone coffin, and use it as a drinking-trough for cattle, or a cistern for a flower-garden, which is done in various places in the vicinity of old abbeys. A stone hollowed out on the under side might be better than a flat stone; because the depending edges would be a kind of side protection to the skeleton; and might, to-

in town or country; because, though the filtration of the soil will purify the water from matter suspended in it, it will not free it from what is held in solution.

3. They would not attend service in any church or chapel whatever, in the vaults of which there were coffins, or in the floors of which interments had taken place. They would absent themselves from all such places, even if there were no immediate danger, in order, by such means as were in their power, to contribute to the discountenance of a practice by all parties allowed to be attended with disgusting and injurious results.

4. Nor would they live in houses in which the privies were not either rendered water-closets, or placed detached from the house.

5. Nor in a house adjoining an open sewer.

6. Nor would they keep a dead body in the hou e more than five days, or at the most, a week.

gether with the name graven on the upper side, procure more respect from those who should fall upon it accidentally in future ages, in excavating for improvements.

The *space of ground required* for a single interment, and for the interments incident to any given population, requires next to be taken into consideration. If all interments took place in the free soil, if a grave were allowed for each coffin, and the grave were never afterwards to be opened, that is, not opened for several generations, then the space required for cemeteries would be considerable. Thus, supposing graves without head-stones or ornaments of any kind to occupy a surface of 7 ft. by 3 ft. 6 in., and the average area of those having grave-stones or monuments to be 10 ft. by 5 ft., then, making an allowance for grass paths between the graves, and for gravel roads, we may take 8 ft. by 4 ft. as the average space on which to calculate the capacity of a garden or ornamental cemetery. This will give 1361 graves to an acre; and, estimating the deaths in a town population at 3 per cent. per annum, this acre would suffice for a population of 1000 souls for 45 years; or for a population of 45,000 for one year. Taking the population of London to be 1,500,000, this would require 33 acres annually, or the whole of that part of Middlesex not covered by London and its suburbs (128,540 acres) in the course of 3895 years. The average number of deaths annually in England and Wales has been ascertained to be about 336,000, which, at 1361 interments to an acre, would require 247 acres annually; or, supposing three interments in each grave 82 acres per annum. On the supposition that ground once occupied by graves was forever afterwards to be held sacred, and not subjected to cultivation of any kind; the mode of interment which would require so large a sacrifice of surface annually may be considered as impracticable; and, for our present purpose, this is the view we shall take of it. We shall, however, hereafter show how separate graves may be procured, not only for those who cannot afford grave-stones, but even for paupers; and these graves never again opened for generations. In the meantime, the mode of burying several coffins in one grave, provided these coffins are of wood, and layers of soil not less than 6 ft. in thickness interposed, and the graves, when once filled, not opened for generations, appears the best adapted for the present state of things. Supposing that on an average three interments take place in each grave or vault before it is finally closed, this will give upwards of 4000 interments to the acre; and, as the eight public cemeteries recently formed in the neighborhood of the metropolis, and the unoccupied part of the new burial-grounds recently formed by different sections of the Dissenters, contain upwards of 300 acres inclusive of the space occupied by roads and buildings, this will probably supply the demand for two centuries to come, even allowing the population to increase.

The *security of the grave* was, till within these few years, an important part of the considerations requisite to be had in view in constructing cemeteries. In some cases it was effected by surrounding the enclosure by high walls, or other effective fences; sometimes by

constructing central watch-towers for stationary watchmen within; sometimes by employing perambulating watchmen; at others by burying in a grave 15 or 20 feet deep; by burying in a walled grave, covered with an iron grating built into the walls all round, some feet beneath the surface soil, and keeping the surface loose, and planted with flowers or shrubs (which, as the grave could not be disturbed without first taking these up, would by their withered state, when replanted, have told what had been attempted); and sometimes by the very extraordinary mode of letting down over the coffin a ponderous cast-iron box, to remain over it for six or eight weeks, till the body was considered to be so far decomposed as to be unfit for the purposes of the anatomist. The iron box, or case, which had remained whelmed over the coffin, but without touching it, was then disinterred and drawn up by machinery, and the wooden coffin was covered with soil, and the grave completed a second time in the usual manner. Even the poorest families, in some parts of Scotland, went to this extraordinary expense. Fortunately a law has been passed which render these precautions unnecessary, and we shall therefore take no further notice of them.

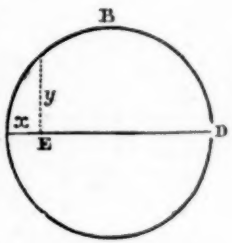
(To be continued.)

Mechanics and Chemistry.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

On the Strength of Cylindrical Steam Boilers.

The circle A B D represents the section of a cylindrical boiler, and it is required to determine its resistance to the pressure of the steam.



Although the pressure is necessarily equal in every point of the periphery, and its direction every where normal to the surface of the cylinder, in consequence of inequalities in the strength of the material, the boiler is most likely to give way at some one point, D for instance, and to open by turning around some other point as a centre of motion. Let A be this turning point, and let us put

F for the force of steam on a unit of the periphery;
x and y for the co-ordinates of the point m, and
δ for the diameter of the cylinder.

Then $F\frac{dx}{ds}$ will represent the force of the steam perpendicular to the line A D on a unit of the periphery of the boiler at the point m, and $F\frac{dx}{ds}ds$ the same force for the space ds. The leverage with which this force acts to turn the section around the point A, is x, and, conse-

quently, the *moment* of this differential force with reference to the point A is

$$\mathrm{F}\,x\,\frac{dx}{ds}\,ds.$$

The horizontal component, or that component of the force at m which is parallel with the line A D, for the space ds will be $\mathrm{F}\,\frac{dy}{ds}\,ds$, and its moment, with reference to the same point A,

$$\mathrm{F}\,y\,\frac{dy}{ds}\,ds.$$

The integral of the sum of these differential quantities, taken between the limits $x=o$ and $x=\delta$ will give the whole moment with reference to A; which integral is expressed by

$$\int \mathrm{F}\,x\,\frac{dx}{ds}\,ds+\int \mathrm{F}\,y\,\frac{dy}{ds}\,ds=\frac{\mathrm{F}\,\delta^2}{2},$$

observing in the operation, that $2\,y\,dy=d.\,(\delta x-x^2)$.

Now, "if P be the cohesive force of the metal of the boiler, and t the thickness of the shell in inches," Pt will represent the resistance which the boiler will offer at D, and P$t.\delta$ the *moment* of this resistance with reference to the point A; which moment, in the condition of equilibrium, must be equal to the tension, or

$$\frac{\mathrm{F}\,\delta^2}{2}=\mathrm{P}t\,\delta;$$

and, consequently, we have for the extreme force per inch which the boiler will bear,

$$\mathrm{F}=\frac{2\,\mathrm{P}t}{\delta}$$

as given by Mr. Latrobe in the last number of the Journal.

We may, if we choose, suppose the material in opposite parts of the section of the boiler, as at A and D, to possess the same strength, so that the rupture will take place at both those points simultaneously,—the upper and lower parts of the cylinder being driven asunder,—and thus avoid introducing the *moment* of the force.

In this case the above integral will be simply

$$\int \mathrm{F}\,\frac{dx}{ds}\,ds+\int \mathrm{F}\,\frac{dy}{ds}\,ds=\mathrm{F}\,\delta,$$

for the expression of the whole tension of the steam of which the effect is to part the boiler at A and D. The resistance which the shell will offer at *each* of those points is Pt—whence the equation of condition

$$\mathrm{F}\,\delta=2\,\mathrm{P}t.$$

and, consequently, as before,

$$\mathrm{F}=\frac{2\,\mathrm{P}t}{\delta}$$

the equation given by Mr. Latrobe in the last number of the Journal.

E.

Iron-Founding.

SECTION I.—The general object of iron-founding is, to mould iron in a melted state into the various forms required for the parts of machines and other constructions. Wrought iron and steel cannot be properly melted by heat. At high temperatures, they drop away and spark off, while the main body of the metal maintains its consistency, and it undergoes rapid oxidation, as is shown by the scales which are perpetually formed on the surface. These metals are, however, in this condition rendered extremely ductile, and the wrought iron especially may be fashioned with facility into any required form by the application of the hammer. On the contrary, pig iron, of which wrought iron and steel are preparations, has peculiarly the property of liquefaction by heat, and is therefore well adapted as a material for castings, in which strength and hardness are required.

The business of the iron-founder is therefore to take advantage of the common law, according to which fluids always find their level. If, for example, a quantity of water be poured into a vessel, however curiously shaped, it first finds the bottom, and then spreads on all sides as it rises, filling every corner it can reach. The body of water must then be a perfect model in form of the interior of the vessel, and this may be seen by solidifying it in its place by the application of cold, and extracting the body of ice.

To mould a quantity of melted iron into any desired form, two things are therefore necessary; first, a model or pattern of the required form; secondly, a substance of sufficient susceptibility and adhesiveness, to receive accurately, and to retain impressions, of that pattern made upon it, against the violence of the liquid iron, when run into the mould which is thereby formed.

Of Patterns.—As to the material of patterns, wood is almost universally employed—yellow pine and mahogany being the kinds principally used. Of these, yellow pine is by far the most in common use. It is very suitable, being very uniform in substance, little interrupted with knots, sufficiently hard, works cleanly and with ease, and, moreover, there is plenty of it. Mahogany does excellently for small patterns, but its expense limits its application to the construction of these. It can be cut very clean, and its superior density and closeness of grain render it well fitted for nice patterns, such as of bushes for journals, small pinions, the teeth of wheels below 1 in. pitch, and in every case of a similar nature, in which the fibres of the wood may be presented endwise to the surface; whereas in working fir in this manner for minute purposes, it is apt to be broken away at the edges.

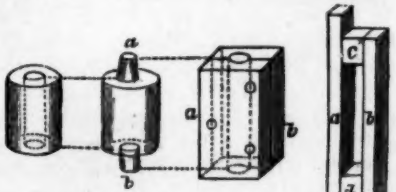
Plane-tree, beach, and red-pine are seldom used. Plane-tree has a very fine and agreeable tissue, and is very suitable for sharp, well-defined patterns, and small patterns intended for constant use, as it retains its sharpness for a long time. Red pine is remarkable for its toughness and straitness of grain, but it is coarse in the fibre. It is only employed for pinning together pieces of wood of deficient dimen-

sions. It does well for purposes of this sort. White pine was formerly much used for patterns, but it has long been almost entirely dismissed as a material for such a purpose, on account of the roughness of its cross-cut.

After an original wood pattern is made, for purposes of light flat moulding, an iron pattern is cast off it, from which afterwards all the moulds are made, as the iron one lasts longer by a great deal than the other, and will preserve its form, which, in a wooden one, is liable to alter in the course of time, especially if the pattern be slender, and the material not thoroughly dry. Tin is frequently used instead of iron, chiefly for patterns of ornamental work, as it can much more readily be cleaned up and smoothed for service. To preserve wood patterns from the effects of damp, a coat of paint ought to be given them, and this is very serviceable in moulding the patterns of wheels, for even during the time of being amongst the sand, which is always damp, the teeth, especially those of smaller pitch, are liable to swell, and have their form destroyed. And as patterns are frequently made of wood not thoroughly dry, they shrink afterwards as they become drier; and those especially which have great extent in proportion to their thickness, such as patterns for plates, will twist at right angles to the direction of the grain, in consequence of the unequal shrinking of the opposite sides. To prevent them from altering their form, bars of wood are nailed across them, which of course leave their impressions in the sand; and it is the care of the moulder to fill these up.

Use of Patterns.—The construction of patterns requires from their nature in many cases to be modified, so as to render the moulding of them practicable. For example, for castings in which recesses or holes passing quite through them, are wanted, it is easy to see that were the space, in many cases actually made in the pattern, to be afterwards occupied by the sand of the moulding, it would carry off the *core* of sand as it is called, altogether. The making of these holes must be provided for in another way. Distinct cores are made by other means, having exactly the dimensions of the hole required in the casting, and that they may be securely held in their positions in the moulding, their ends project into counterpart holes in the sand, and are there fixed. These holes are formed by corresponding projections made in the pattern, named *core-prints*. For example, were it required to cast a coupling for shafts, of a cylindrical form, 12 inches deep, by 8 inches diameter outside, and 4 inside diameter, as sketched (fig. 1), a pattern of the same size, (fig. 2) is made, and two prints, *a*, *b*, are put on, in the proper positions to support the core; this is made of sand in a *box*, shown by fig. 3, which is simply two thick pieces of wood, *a*, *b*, held together by wooden pins. Into each of these, half the core hole is cut, so that when the core is formed in it, it may easily be got out by separating the

Fig. 1 Fig. 2 Fig. 3 Fig. 4



halves. If then after the pattern (2) has been moulded, the core formed in the box (3) be inserted in the recesses left for it by the prints, the casting (1) will be formed, which otherwise would have been solid; for, were a pattern made like fig. 1, with the hole made through it, on withdrawing it from the sand, it would, most likely, carry off the *core* with it. In this instance, indeed, with much care on the part of the moulder, he might have managed to leave the core in its position unbroken, on account of its considerable diameter; but if it had been much smaller, say 2 inches, it could not have stood the tug of the pattern, and much less the shock of the melted iron. But, further, it is quite impracticable when the core lies horizontally, as the pattern in being withdrawn would, of course, unavoidably lift it away.

Thus, by means of distinct cores formed by boxes, holes and recesses of every kind are made in castings, if they be not already formed in the pattern.

Square cores are formed by two slips of wood, *a*, *b*, fig. 4, of the required thickness, and kept apart at the ends, by two pieces *c*, *d*, forming, by filling the space within with sand, the core required. It is, however, foreign, to our present purpose to describe the construction of patterns, further than is necessary to the elucidation of the subject under consideration.

Materials used in Moulding.—The principal materials used in the various branches of moulding, are sand of various kinds, clay, blackening, coal-dust, and cow-hair.

Sand is superior to all other substances as a material for forming moulds generally. For, in the first place, the hot iron has no chemical action upon it, though certainly it acts upon the matters which it is found necessary to associate with it, namely, blackening and coal. But secondly, it operates well as a conducting medium for the air expelled from the space filled by the iron, and for the other gases generated by the action of the heat on the blackening and the coal. And thirdly, it possesses considerable adhesiveness when rammed together —sufficient indeed, to make it retain its form against the pressure of the melted iron; and, moreover, it is easily made to conform itself very accurately to the surface of the pattern imbedded in it.

The sand of the London basin is the finest in the country. It is universally employed in the manufacture of fine goods, as grates, fenders, and the like. The sand in the neighborhood of Falkirk is coarser and opener in the pores, which unfits it for such work. It is employed for casting hollow ware—pots and kettles, for example, as the inclosed air escapes freely through the inside body of sand in the moulding of such articles. It affords a beautiful, smooth skin to the castings from Scotch iron, so remarkable in the hollow goods of the Carron Iron Works, in Stirlingshire, and of the Phœnix Iron Works, at Glasgow. The Belfast sand is finer than that from Falkirk, and is used principally for fine machinery castings. It is also sometimes used for facing the moulds of ornamental work, to give a fine surface. It is besides excellent for hollow moulding, when mixed with the Falkirk sand; but it is too expensive for general adoption in that way. It is a mixture of a very fine adhesive sand, and an opener

kind. Rock-sand, the debris of abraded rock, and free-sand from the sea-shore are employed for making cores. The former, by itself, does very well for short cores, which open into the sand of the mouldings at both ends, as it contains a proportion of clay in its composition, which gives it cohesion. But it requires to be moderated with free-sand, to make it opener for the better escape of the air in its pores, when used for cores of considerable length, which, of course, are surrounded on all sides by the iron, except at the small portions at the extremities, by which alone the air can find exit. Free sand is also used alone for such cores, but as it wants adhesiveness, it requires to be tempered with clay water, barm, or the refuse of pease meal. In the use of the last, accuracy is required in proportioning it. The first is used in ordinary cases, and the barm only in very particular cases.

Clay is also very much employed, when mixed with sand, for loam-moulding. These ingredients are ground together with water, to give them consistency, and their proportions generally are one part of clay to eight or nine parts of sand. This, with a handful of hair mixed with it, forms ordinary *loam;* and a shovelful of horse-dung, seeds, or saw-dust, is added for *core-loam.* The purposes of these minor elements will be afterwards referred to.

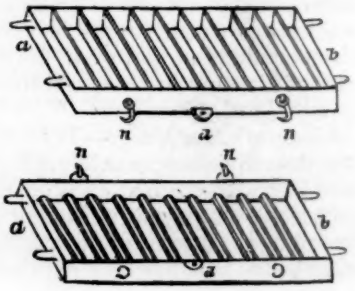
Blackening and coal-dust are employed to resist the penetrating action of the iron on the sand. Blackening is simply charred oak wood ground to powder. Oak charcoal is superior to all the other ordinary wood charcoals for the purpose, as it is the heaviest. Other wood charcoals are apt to be disengaged from the surface of the mould to which they are applied, and to float in the iron while liquid, which of course defeats the object of their use. According to Mr. Mushet's experiments, oak produces 22.6 per cent., that is, fully one-fifth of its weight of charcoal. Were the iron allowed to come into direct contact with the sand of the mould, it would enter its minute interstices, and thus yield but a rough surface. To avoid this, blackening is dusted over the surface of the mould, pressed down on it, and smoothed, in cases of green sand castings, but it is mixed with clay-water, for covering loam-mouldings. Its essential property as a protector of the sand, is its inflammability. All combustible solid substances peculiarly resist liquid iron, as may be exemplified in pouring it over a smooth surface of wood. It rolls about as lively as mercury, on account of the continued effusion of gaseous matter by the combustion of the wood heaving up the iron from the surface. Now, in cases of heavy castings in green sand, as the action of the metal becomes too powerful for the blackening, this is assisted by coal-dust, which is mixed uniformly in the sand. It is never more than one-tenth of the sand in bulk, and the best kind of coal for the purpose, is the rich, hard, splint coal.

Kinds of Moulding.—The art of moulding may be divided into two great divisions; namely, green and dry-sand moulding, and loam-moulding. In the first division, patterns of the articles wanted are universally employed in forming the mould; in the second division, the ordinary patterns are dispensed with, the objects of this division being heavy castings of a regular form; as cylindrical bodies gener-

ally, and other circular ware, such as sugar-pans and gas-retorts. Large square vessels, water-tanks, for example, may also be made by a process of loam-moulding. The first division again embraces every other variety of article, for which there must be patterns. Dry-sand moulding is generally employed for the making of pipes, columns, shafts, and other long bodies of a cylindrical form. It is firmer and better adapted to purposes of this kind than green sand. The material of dry sand, is the loam already used in loam-moulding, called *pit-sand*, mixed in the mill with an addition of rock-sand. It is named dry-sand, in contradistinction to green-sand, because, after being moulded, it must be dried by heat to fit it for the purpose; whereas the latter is employed as it comes from its native bed, new and damp; the dampness indeed is assisted afterwards when necessary, as a certain degree of it is always requisite.

The operations of green-sand moulding are generally recognised under two great classes—hollow moulding, and flat moulding. The former includes pots, frying-pans, and every other kind of cooking ware, of a light dished form. The latter class is very extensive, and is so termed in opposition to hollow-moulding. It includes all objects of a flat nature, the various parts of grate furniture, for example, and other ornamental work generally, stoves, roans, smoothing-irons, all kinds of machinery that do not fall under loam and dry-sand moulding, for instance, all the cast-iron work of spinning and loom machinery. In fact, a kind of subdivision exists, known as job-moulding —a homely term, including machinery generally, and the heavier kind of work, distinguishing them from the ornamental and the other lighter work. A steam-engine affords in the parts of it, examples of the three kinds of moulding. The steam-cylinder and the air-pump which are round, and the condenser, which is often square, are instances of loam-castings,—the fly-wheel shaft, and the single columns supporting the framing are examples of dry-sand castings, and the beam, sole-plate, entablature, and connecting rod, if of cast-iron, are referable to the heavier green-sand casting. The cistern plates, too, are decided instances of flat-moulding.

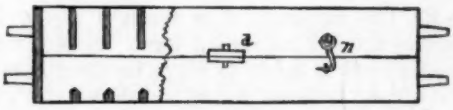
Processes and Tools.—The processes of green-sand moulding, and the tools employed in it, claim our first attention. In processes of green and dry-sand moulding, boxes are always employed, the purpose of which is, to contain the sand in which the pattern is moulded. These boxes are for convenience of various sizes. If there be a great or constant demand for castings of one form, boxes are made expressly for them, corresponding in form. By this plan, a saving of labour is effected, as the ramming up of useless corners with sand is avoided. For general purposes, boxes are made rectangular, and in two halves, as shown in the sketch annexed. These boxes have neither top



nor bottom, but each half-box, or more correctly each box, is composed of an outside rectangular frame *a*, *b*, which is generally 3, 4 or 5 inches deep for the lighter flat-moulding. They have transverse ribs joining the opposite sides at equal distances of 4½ inches between them. The object of their being open on the upper and under sides is, to allow the application of the tools for ramming the sand in the box; the ribs being at the same time sufficient as holding surfaces for the sand, which is formed into a close adhesive mass by the ramming, and, in a manner, dovetailed into the ribs. The rougher, therefore, these boxes can be made, the better—they hold the sand more effectually, and, accordingly, in casting the boxes themselves, the patterns for them are simply laid in the sand on the ground, and after being rammed, are drawn out. There is no blackening used for the surfaces of the moulding, and thus the iron enters the pores of the sand, and roughens.

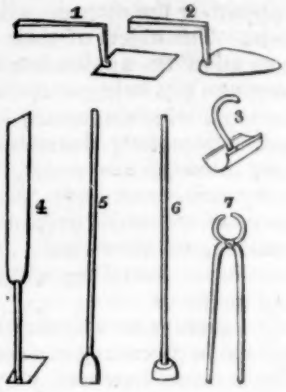
As there is no covering for the mould, it being exposed to the air, this mode of casting is named open-sand casting. The exposed surface is made, however, very irregular and rough, so that this mode of casting is used only for moulding boxes, when the roughness is a virtue, and for articles of a coarse nature.

The figure annexed is a longitudinal view and partial section of a pair of boxes, in which it is seen that the ribs of the upper box are not so deep as the outside frame. They are generally an inch less deep to allow a depth of sand over the pattern that is imbedded in the sand of the lower box. The frame of this box, called the drag-box, is the same as that of the upper, but the ribs are much shorter and thicker, as it is not required to be moved about and inverted like the upper one; besides, it allows much more available depth of space for the moulding of the pattern. As the lifting and shifting of these boxes, when small, is usually managed by two men, they have two snugs or handles at each end, seen in the first figure, by which they are held. They have also usually three hooks and eyes, *n*, *n*, and three pins and holes to receive them, arranged alternately along the sides, there being two on either side, and one over the other. The pins are fixed on ears, *d*, *d*, *d*, cast on the sides of the drag box, and pass through holes made in ears on the upper box, which correspond, so that the boxes in being placed and replaced together, must have always the same relative position. The hooks and eyes hold them tightly together for casting.



The sketches annexed represent the different kinds of tools employed by flat-moulders in the execution of thin work. No. 1, is the trowel—the instrument in most frequent use by moulders. There are various sizes of it used, from ¼th to 2 inches broad in the blade, and 3 inches long generally. The purpose of the trowel is to clean away and smooth down the surface of the sand, to press down and polish the blackening, repair injured parts of the moulding, and so

on. No. 2, is another form of trowel of a heart-shape. It is particularly employed for entering acute angles in a moulding, into which the square trowel evidently cannot go. No. 3, is another form of tools for managing hollow impressions in the sand. No. 4, is the form of the sleeker and cleaner. As the trowel is applicable only to open plain surfaces, this tool is used for cleaning and smoothing sunk surfaces in the sand which the ordinary trowel cannot reach—as the impression of a flange, or of any flat part of a pattern presented edgewise to the sand. The upper end is applied to the sides of such an impression for sleeking or smoothing it, and the under end goes to the bottom, where it is used both for taking up loose sand lying there, and for pressing and smoothing down the surface. It is to be noticed too, that the upper end is presented edgewise to the direction of the *spade* at the under end, so that when this is employed at the bottom of a deep recess, the upper end stands sideways to the side of the recess and permits free motion. No. 5, is the first rammer; it is about 4 feet 6 inches long, and its under face is about 2 inches + 1 inch. Sometimes the upper end, by being tapered off, is made to serve for forcing holes in the sand. No. 6, is the second rammer for finishing the work of the first. It is round in the face, about 3½ inches diameter, with a wooden shank of convenient length. No. 7, represents the pincers used for laying hold of and shifting about the castings. They have no peculiarity except in having their holding faces round and flat.



Besides these tools, shovels are used for working the sand, sieves and riddles for refining it, and bellows for blowing off loose sand from mouldings; pots for holding the parting sand and the water used in moulding, swabs for applying this water to the mouldings, being simply tufts of tow brought to a point, and separate linen bags of pease-meal and blackening, through the texture of which these materials are shaken on the sand. There are also piercers or "pricKers," as they are named, being pieces of thick iron wire sharpened at one end to a point, for piercing the sand to let off air.

To be Continued.

"*An Experimental Inquiry as to the Co-efficient of Laboring Force in Overshot Water-wheels, whose diameter is equal to, or exceeds, the total descent due to the fall; and of Water-wheels moving in circular Channels.*" By Robt. Mallett, M. Inst. C. E.

This paper is partly mathematical, and partly experimental. The investigation which it details, the results of which are given in ten

tables of experiments, had in view, principally, to obtain the definite solution of the following questions.

1st. With a given height of fall and head of water, or, in other words, a given descent and depth of water in the pentrough, will any diameter of wheel greater than that of the fall give an increase of laboring force (*i. e.* a better effect than the latter), or will a loss of laboring force result by so increasing the diameter?

2nd. When the head of water is necessarily variable, under what conditions will an advantage be obtained by the use of the larger wheel, and what will be the maximum advantage?

3rd. Is any increase of laboring force obtained, by causing the loaded arc of an overshot wheel to revolve in a closely fitting circular race, or conduit? and if so, what is the amount of advantage, and what the conditions for maximum effect?

The author briefly touches upon the accepted theory of water wheels, the experimental researches of Smeaton, and the recent improvements in theory, due to the analytic investigations of German and French engineers.

Smeaton, in his paper on water wheels, read to the Royal Society in May, 1759, and Dr. Robison, in his treatise on water wheels, lay down as a fixed principle, that no advantage can be obtained by making the diameter of an overshot wheel greater than that of the total descent, minus so much as is requisite to give the water, on reaching the wheel, its proper velocity.

The author, however, contends that while the reasoning of the latter is inconclusive, there are some circumstances which are necessarily in favor of the larger wheel, and that conditions may occur in practice, in which it is desirable to use the larger wheel, even at some sacrifice of power; and that hence it is important to ascertain its co-efficient of laboring force, as compared with that of the size assigned by Smeaton for maximum effect.

The author states, first, the general proposition, "that the laboring force ("travail" of French writers,) or "mechanical power" of Smeaton, of any machine for transferring the motive power of water "is equal to that of the whole moving power employed—minus the half of the *vis viva* lost by the water on entering the machine, and minus the half of the *vis viva* due to the velocity of the water on quitting it." He deduces from the theory, the following results, coinciding with the conclusions obtained by experiment.

1st. If the portion of the total descent passed through by the water before it reaches the wheel be given, the velocity of the circumference should be one-half that due to this height.

2nd. If the velocity of the circumference be given, the water must descend through such a fraction of the whole fall before reaching the wheel, as will generate the above velocity.

3rd. The maximum of laboring force is greater, as the velocity of the wheel is less; and its limit, theoretically, approaches that due to the whole fall.

General equations are given, expressing the amount of laboring force in all the conditions considered, and their maxima.

One of the principal advantages of using an overshot wheel greater in diameter than the height of the fall, is the power thus afforded, of rendering available any additional head of water occurring at intervals, from freshes or other causes, by admitting the water upon the wheel at higher levels.

The first course of experiments is dedicated to the determination of the comparative value of two water wheels, one of whose diameter is equal to the whole fall, and the other to the head and fall, or to the total descent; by the head, being in every case understood, the efficient head, or that due to the real velocity of efflux at the shuttle, as determined according to Smeaton's mode of experimenting.

The apparatus employed in this research consisted of two accurately made models of overshot wheels, with curved buckets. These were made of tin plate, the arms being of brass, and the axles of cast iron. Special contrivances were adopted to measure the weight of water which passed through either wheel during each experiment, to preserve the head of water strictly constant, and to determine the number of revolutions, and the speed of the wheels.

One wheel was 25.5 inches diameter, the other, 33 inches diameter. The value of the laboring force was determined directly, by the elevation of known weights to a height, by a silken cord over a pulley; the altitude being read off on a fixed rule placed vertically against a lofty chimney; and in other experiments, relatively by the speed of rotation given to a regulating fly or vane. The depth of the efficient head was 6 inches in all cases.

The weight of water passed through either wheel in one experiment, was always 1000 pounds avoirdupoise.

All the principal results given in the table accompanying the paper, are the average of five good experiments; from the large scale upon which these were conducted, the accurate construction of the apparatus, and the care bestowed upon the research, which was undertaken with reference to an actual case in the author's professional practice, he is disposed to give much confidence to the results.

The weight of water contained in the loaded arc of each wheel is accurately ascertained, and in the tables which accompany the paper, the results of the several experiments are given at length.

The velocity of the wheels, under different circumstances, is carefully noted and discussed with respect to the maximum force.

The author next ascertains the value of the circular conduits, and states that generally, in round numbers, there is an economy of laboring force, amounting to from 8 to 11 per cent. of the power of the fall, obtained by the use of a conduit to retain the water in the lower part of the buckets of an overshot wheel, whose diameter is equal to the fall. The velocity of a water wheel working thus, may vary through a larger range without a material loss of power, and a steady motion is continued to a lower velocity than when it is working in a free race.

The author finally arrives at the following general practical conclusions:—

1st. When the depth of water in the reservoir is invariable, the diameter of the water-wheel should never be greater than the entire height of the fall, less, so much of it as may be requisite to give the water a proper velocity on entering the buckets.

2nd. When the depth of water in the reservoir varies considerably and unavoidably, an advantage may be obtained by applying a larger wheel, dependent upon the extent of fluctuation and ratio in time, that the water is at its highest and lowest levels during a given prolonged period; if this be a ratio of equality in time, there will be no advantage; and hence, in practice, the cases will be rare when any advantage will obtain by the use of an overshot wheel, greater in diameter than the height of fall—minus, the head due to the required velocity of the water reaching the wheel.

3rd. If the level of the water in the reservoir never fall below the mean depth of the reservoir, when at the highest and lowest, and the average depth be between an eighth and a tenth of the height of the fall, then the average laboring force of the large wheel will be greater than that of the small one; and it will, of course, retain its increased advantage at periods of increased depth of the reservoir.

Dr. Robison's views, therefore, upon this branch of the subject, should, he contends, receive a limitation.

A positive advantage is obtained by the use of the conduit varying with the conditions of the wheel and fall, of nearly 11 per cent. of the total power.

The value increases with the wheel's velocity up to 4½ feet per second, or to 6 feet per second, in large wheels. Hence, he argues, that it is practicable to increase the efficiency of the best overshot wheels, as now usually made, at least 10 per cent. by this application. The only objections urged against the use of the conduit are of a practical character, relating to the difficulty of making it fit close, of repair, &c.; but however these may have applied to the rude workmanship of the older wooden wheels, with wood or stone conduits, they are unimportant, as referring to modern water-wheels made of iron. The conduits may be also made of cast-iron, provided with adjusting screws, and hence of being always kept fitting, readily repaired, and capable of being withdrawn from the circumference of the wheel in time of frost, &c.

The paper is illustrated by a drawing, showing the elevation and partial sections of the experimental apparatus, and a diagram showing the full size of the loaded arc of each model.

Mr. Farey observed, that the result arrived at by the experiments, appeared to correspond nearly with those recorded by Smeaton, who had experimented upon, and used practically, both kinds of wheels. The buckets of the model wheels used in the experiments did not appear to be of the best form, and they were entirely filled with water; hence an apparent advantage had been obtained, by the use of the circular conduit to retain the water in the buckets. But that would not be realized in practice, for as the form of the bucket regulated the point at which the water quitted it, and it was the practice of the modern millwrights to make the wheels very broad, in order that the

buckets should not be filled to more than one-third of their depth, the circular conduits became less useful, and in fact were now seldom used. Smeaton's practice was, to entirely fill the buckets with water, but he never adhered to the slow velocity of revolution which he recommended theoretically in his paper to the Royal Society.

Mr. Fairbairn had adopted broad wheels with an improved form of bucket, partially filled, and had obtained a more regular motion, particularly at high velocities.

Mr. Farey promised to present to the Institution, a copy of the method of calculation adopted by Smeaton for water-wheels.

Mr. Taylor corroborated Mr. Farey's statement of the advantage of using broad wheels, with the buckets of a fine pitch and partially filled; circular conduits then became unnecessary: this was practiced among the millwrights in North Wales with eminent success, and a velocity of six feet per second was given to the wheel.

Mr. Homersham believed that in Smeaton's latter works he increased the velocity of his wheels to six feet per second.

Mr. Rennie gave great credit to the author for the ingenuity of the apparatus with which the experiments were tried, and for the clearness of the tabulated results; but owing to the necessary limited size of the model wheels, he feared the results could not be relied upon for application in practice to large wheels. The experiments of Borda, Bossut, Smeaton, Banks and others, were all liable to the same objection.

The best modern experiments are those by the Franklin Institute, by Poncelet, and by Morin.

The result of these might be thus:

Undershot wheel,	the ratio of power to effect	varied	from	0.27	to 0.30
Breast wheels,	"	"	"	0.45	to 0.50
Overshot wheels,	"	"	"	0.60	to 0.80
Average,	"	"	"		0.60

The velocity of the old English water-wheels was generally about three feet per second; the American wheels four feet, and the French wheels six feet: this latter speed was now adopted by the best millwrights in England. Mr. Hughes, at Mr. Gott's factory at Leeds, and Mr. Fairbairn, had found advantage from it; the latter also had a particular contrivance for carrying off the air freely from the buckets.

It was important to regulate the thickness of the sheet of water running over the shuttle upon the wheel; four to five inches was found in practice to be the maximum depth allowed.

The object being to utilize the greatest height of fall and the greatest available quantity of water, by means of properly constructed openings and such sluice-gates as were first introduced by the late Mr. Rennie for the breast-wheels constructed by him, instead of penning up the water in a trough, it was made to flow in a sheet of regular thickness over the top of the shuttle, and by a self-regulating apparatus to adjust itself at all times to the height of the water; thus obtaining the advantage of the full height of the fall at its surface, and obviating the necessity for the apparatus proposed by Mr. Mallett.

Mr. Mallett begged to dissent from the validity of the objections which had been made to the practical value of his experiments. With respect to the form of the bucket, that used by him could not, he contended, be called a bad form, although it might be susceptible of improvement; but as the experiments were altogether comparative, it was foreign to the question whether the form was bad or good, the same having been used in both wheels.

As it was shown that a certain relation subsisted between two water-wheels with the same total descent, but with different diameters, as to their co-efficient of laboring force, a proportional relation would exist with any worse or better form of bucket. The results considered as absolute measures of effect, being obtained with a form of bucket which approached nearer to the best forms now in use, than did those of Smeaton, or any other experimenter, were more applicable to modern practice, and therefore he must consider his results, as not without utility.

With regard to the custom of only partially filling the buckets, it must be remarked that buckets of the best forms begin to spill their contents before arriving at the lowest point of the loaded arc; the partial filling could, therefore, only palliate the evil which the circular conduit was designed to remedy. He must, however, contend that a positive disadvantage attended the partial filling. A permanent loss of fall was produced equal to the distance between the centres of gravity of the fall, and of the empty portions of the top bucket at the moment it had passed the sluice; this distance could be but little varied by the fineness of pitch of the bucket, and depended more on the depth of the shrouding. That there was a constant loss of laboring force by a practical diminution of the effective leverage, or a reduction in the "moment" of the loaded arc. That as the wheel revolved, the centre of gravity of the fluid contained in each bucket, as it approached the lower portion of the loaded arc, was transferred to a greater distance from the centre of motion even before the contents commenced spilling; but the angular motion of the centre of gravity of any one bucket was at first that due to its distance from the centre of motion of the wheel, or to its radius; and as the radius increased, a greater angular velocity would be acquired by the water which had changed its position on approaching the lower point of the wheel; but this increased velocity was given at the expense of the power of the wheel, and hence a partially filled bucket would, he contended, be always attended with a loss of laboring force. To the last objection, a full bucket was not liable.

From all these reasons, he felt justified in concluding, that the use of the circular conduit was more advantageous than the practice of partially filling the buckets.

With respect to the shuttle delivering the water over the top, where the head of water and the fall were constant, no advantage could be obtained by the use of a wheel greater in diameter than the total descent; it was assumed that this form of shuttle would be used in order always to deliver the water as high as possible upon the periphery of the wheel; but the question was, "If the head be variable, what

should be the diameter of the wheel to secure the best effect?" The paper showed that a wheel whose diameter was equal to the total descent, when the head was a maximum, did not always give the greatest average laboring force. The question was therefore independent of the sort of shuttle used; it assumed the power of always admitting the water upon the wheel at the highest point of the total descent, and sought to establish the best relation between the diameter of the wheel and the whole descent when the head alone was variable, according to given conditions. The results of this part of the investigation, therefore, while they admitted the full value of Mr. Rennie's shuttle, went further, and pointed out the limits of its useful application.

He was fully aware of the prejudice which existed against the circular conduit, and once participated in it; but his attention had been forcibly drawn to it in his practice, and having used them very beneficially upon wheels of 40, 50, and 60 horses' power, which he had constructed for mining purposes, he wished to draw the attention of the profession to the consideration of their practical merits when adapted to good wheels. Civ. Eng. & Arch. Journ.

Experiments and Observations on Möser's Discovery, proving the Effect is neither due to Light nor Heat. By Horatio Prater.

It is proposed now to demonstrate, that the radiation discovered by Möser is not invisible light, as he supposes, nor heat, as has since been supposed. For, first, where is the evidence that bodies absorb light? Some few, certainly, have been shown so to do; but surely not the metals, &c. &c., which exhibit the greatest facility in receiving and giving the impressions discovered by Möser. It seems, *à priori*, more probable that the radiation in question should consist of heat (which we know exists in all matters) than of light. Accordingly, Mr. Hunt has written an elaborate paper in favor of the supposition that such radiation consists of heat. In the course of this essay, however, it will appear, that neither of these suppositions is correct.

1. *With regard to the nature of the substances that produce spectra.*—Every substance I have tried has produced its spectrum when left on a polished copper plate. Coins, whether of gold, silver, or copper, platinum, nickel, brass, pieces of glass, wafers (red, blue, or white), peppermint or rose drops, whalebone, talc, gum, a horsehair ring, lava from Vesuvius, Indian rubber (but slight), and sealing wax. This last, left ten days, gave a whitish grey *permanent** spectrum, clearer than any of the others, though the wax and plate were both kept dry as usual. The impression on a small brass seal

* By a *permanent* spectrum is always meant, in this essay, a spectrum that remains when the substances or coins are removed—not a spectrum which cannot be rubbed off by *gentle* friction, for all the above *permanent* spectra are yet soon effaced by friction.

(a P) was very obvious when the plate was breathed on. The seal had been left ten days.*

2. *Effects of Dissimilar Metals.*—It has been asserted, that when a gold or silver coin is placed on a copper plate, the effect is greater than when a copper coin, &c. is placed on the same metal. When heat is used, this position is true, as will be shown hereafter; but when the plates and coins are both kept cold, (exposed to external air, for instance, in March,) a farthing, on two different occasions, in an hour, left as good a spectral image as a sovereign,—I thought, a better one.

It was, however, remarkable, when a heat of 160° was applied to this plate, that the spectrum of the copper soon became invisible, while that of the gold was apparently not at all diminished. This experiment was repeated twice with the same result. I likewise found that, though the spectrum of the copper was to appearance, *at first*, as good as that of gold or silver, yet that it began to disappear much sooner, after a few breathings on the plate, than did the spectrum produced by gold or silver. *On the whole*, therefore, it seems right to admit that the effect is greater when dissimilar metals are used.

3. *Effect of Unequal Heat on the Plate and Coins.*—It has also been asserted, that when the copper coin is heated, and the metal plate of copper kept very cool, that the effect is increased. I have, however, not been able to satisfy myself of the truth of this statement. A penny and a farthing, heated to between 130° and 160°, and laid on a cold copper plate half an hour, did not appear to leave even so good a spectrum as two of the same coins left to cool for half an hour outside the window, by the side of the plate itself, before being placed on the plate. All the coins were placed on the plate at the same time, and left the same time. Neither could I perceive any difference when one sovereign was heated and the other not, both being placed on the same copper plate.

4. *Effect of Heat Generally.*—In order to ascertain whether heat hastens the impression, the following experiments were made:—1. A bright half-sovereign, a bright half-penny, and a dull one, were heated to about 150° on polished copper plate. The half sovereign left a *permanent* impression; and both the halfpence left spectra visible only by breathing. It was obvious from this experiment and others, that heat increases the effect where *contact is permitted*,† since

* It left a permanent spectrum of its *margin*. Coins left a similar time do the same; *the part where they have remained retaining its polish*. The permanent spectrum then, in such cases, plainly depends on the substances preserving the plate from oxidation by contact or proximity. I add proximity, because a half-crown or penny resting on a fourpenny piece, placed on the plate, likewise leaves its *permanent* spectrum. The free circulation of the air is impeded here in consequence of the extreme proximity, just as it is by actual contact. Hence the oxidation being less in all such cases than in the parts external to the coins, we have of necessity the permanent spectra.

† Although the mark is permanent in such cases, still it very easily rubs off, even when gold has remained five hours on heated copper plates; and no spectral figure is left when the part is breathed on, after the plate has been well rubbed. As this is the case, such permanent mark is not to be considered as a *different* effect, but only as a *higher degree* of the same effect as that caused by mere imposition without heat. I found all the things mentioned in Section 1. gave a *permanent* spectrum if left eleven days, but only one rendered visible by breathing, being left but a few hours.

the impression is permanent. Accordingly it was deemed right to try if heat has this effect when the coin is at a distance from the copper plate.

I put a silver fourpenny piece on the plate, and on the fourpenny piece I put a penny. I found that when these remained only twenty-four hours, that no spectral image of the penny was produced; but on remaining forty-eight hours one was apparent. In this last case, the lettering of the fourpenny piece became almost visible when breathed upon, but without this no mark of it was perceptible. The penny piece, however, left its mark without being breathed upon—an annular *bright* mark, which was not rendered more or less distinct by being breathed on. The spectrum of the fourpenny piece was alone brought into view by this.* The place where this had laid was exactly as bright as that covered by the penny. In fact, the copper plate seemed preserved from oxidation by the contact and proximity of these coins. Thus, then, it appeared to require forty-eight hours for a spectrum of the penny piece to be produced—the spectrum of a coin *not in contact*. The same experiment being made at a heat of 160°, no spectrum of the penny appeared after one hour, though the fourpenny piece had left a strong impression.

Ditto, continued for five hours, a spectrum of the penny was *just* visible, and only so when the plate was held in a particular position with regard to light.

A half-crown piece being laid on a half-sovereign, and the same heat continued five hours on the same plate, the half-sovereign left a still better impression than the fourpenny piece† above mentioned, and the half-crown had also made a *permanent* spectrum very visible.

A farthing, which had rested the same time *on* the plate, left no permanent spectrum, but only one slightly visible by breathing. Even when pressed upon by two pence, and left eight hours, it left only a *barely visible* permanent spectrum: so a brass medal. These spectra being rendered far more visible by breathing, could hardly be considered permanent spectra.

These experiments show:—1st. That heat much increases the rapidity of the radiation, *even when the object is not in direct contact;* and 2ndly. That it takes place much more energetically from gold and silver than from copper (a copper plate being used.) They also show that a permanent spectrum is to be considered only as a *higher degree* of that produced or rendered apparent by breathing.

A sovereign, two hours on a very thin lamina of talc, at the above heat, gave no spectrum; talc alone gave its spectrum; nor did a halfpenny, eight hours on the same at the same heat; nor a shilling (new) on a thin piece of glass, the shilling being under a halfpenny.

*However, after six or eight days, *as this began to tarnish*, the spectrum of the fourpenny piece became visible without breathing on it. Yet nothing had been done, except that the plate had been heated to about 150° once or twice for other experiments.

†When the plate was rubbed pretty strongly with chamois leather only, the spectra of the half-sovereign and four-penny piece were soon effaced; while those of the half-crown and penny (not having been in contact with the plate) remained.

The talc and glass in these cases alone gave a spectrum; the talc a better and more permanent one than the glass.* I should have said the talc was on copper-plate.

The spectrum of the penny, in the experiment lately detailed, is equally visible when the experiment is made on glass; but polished metals seem to show it the best.

When glass is used, there is, after from twenty-four to forty-eight hours, a slight deposition of dust, &c. around the parts which are not covered by the penny, and thus a round mark (permanent spectrum) is visible on removing the penny, even before breathing at all; still on rubbing it off *till nothing is visible*, and breathing on it again, the spectrum of the penny appears, as well as of the fourpenny piece, proving that dust adheres much more strongly than we should have supposed, or perhaps better—leaves its mark behind with greater pertinacity.

That this is the true explanation of the appearance of a spectrum, when the coin is not in direct contact with glass, was to me rendered clear by another experiment, in which a half-crown was left on one sixpence, and a penny on another, on a clean glass plate *covered over with paper, and kept in a closet* for ninety-six hours; yet on examination, neither a permanent spectrum, nor even an evanescent one by breathing, was perceptible either of the half-crown or penny; the sixpences alone had left spectra (which, however, were only visible by breathing), that under the half-crown being the clearest. Yet the penny and half-crown were in the best condition for giving spectra, for the surfaces of both were tarnished, and that of the copper purposely so.

This result induced me to try the same with a copper plate, and I found that when a bright half-crown (having been well boiled in water and then polished) was placed on a fourpenny piece, similarly treated, and left forty-eight hours *covered* in the closet as above, that the half-crown left no spectrum, even evanescent. Neither did a *purposely* tarnished penny placed on another fourpenny piece, and left the same time.

5. *As regards the Distance from the Plate at which Images may be taken.*—A silver fourpenny piece is about the one-twentieth of an inch in thickness, and at this distance we have seen silver, copper, and of course gold, give a spectrum image on a copper plate. But on putting a half-crown and two sixpences and a half-franc piece, making the distance from the plate more than one-tenth of an inch, no spectrum of the half-crown was made, although the experiment was continued for twelve successive days and nights. Neither was any made by removing the half-franc piece (thus making the distance only one-tenth of an inch), and continuing heat at 160° or so for five hours.

A sovereign fixed at three-quarters of an inch, and a small brass medal at somewhat less than half an inch, from a polished copper

* A sovereign on a silver four-penny piece two hours, gave only a very feeble permanent spectrum; the silver leaving, of course, a well marked spectrum.

plate, and continued in such position for seventeen days and nights in a little closed deal box, gave not the least vestiges of spectra; neither did a fourpenny piece left at one-fifth of an inch, nor a card plate (engraved) left the one-tenth of an inch, for eleven days. The copper plate had remained *perfectly polished* in both experiments; and this is worthy of remark, as showing that in *confined* air copper does not oxidate perceptibly. Another plate left in the *same* room was completely tarnished in five or six days.

A fourpenny piece, about the one-twentieth of an inch, under a silver plate for eleven days, gave scarcely a perceptible spectrum; though a farthing, on which the plate *had rested*, gave a good spectrum, but not a *permanent* one, (*i. e.* breathing was required to show it.)

A fourpenny piece is about the one-twentieth of an inch in thickness, and this seems the greatest distance an image can be taken by the above plan. But even at this distance I have not succeeded, if the half-crown laid on the fourpenny piece is *perfectly* polished, and *all external dust, &c,. carefully excluded* by the box just mentioned—(see Sec. 8, on the comparative polish of metals).

Lond. Athenæum.

(To be continued.)

Preparation of Bicarbonate of Soda. By M. Artus.

According to the author, the property of charcoal to condense gases may be turned to good account in the preparation of bicarbonate of soda.

2 parts of effloresced carbonate of soda are mixed with 1 part freshly heated pulverized charcoal (from soft wood), the mass moistened with some water, and then brought into a high cylinder, and carbonic acid passed into it. This gas is allowed to act for 24 hours, when the mass is taken out, pulverized, some water added to it, and again returned into the cylinder, and carbonic acid passed into it. The same operation is twice repeated, the mass is then taken out of the cylinder and treated with 8 parts of hot water, filtered while hot, and the solution left to crystalize. The bicarbonate of soda formed crystalizes from the solution, while the neutral carbonate remains dissolved in the mother-ley, which is poured off, and may be employed for other purposes. The residuous bicarbonate of soda is then washed with a little cold water, to remove the last traces of any adhering carbonate, and is then dried.

In this manner somewhat more than two-thirds of the quantity of neutral carbonate employed, is obtained of bicarbonate of excellent quality. The charcoal assists greatly in the absorption of the carbonic acid gas, hastening the operation, and at the same time the neutral carbonate of soda becomes of a beautiful white by contact with the charcoal.—*Allgem. Pharmaceutishe Zeitschrift* von Artus, No. I.

Lond. Chem. Gaz.